PROCEEDINGS
OF THE
American Philosophical Society
HELD AT PHILADELPHIA
FOR PROMOTING USEFUL KNOWLEDGE

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THE AMERICAN PHILOSOPHICAL SOCIETY
Independence Square
1939

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PHILADELPHIA
THE AMERICAN PHILOSOPHICAL SOCIETY
Independence Square

INTRINSIC AND ENVIRONMENTAL FACTORS IN AMERICAN POPULATION GROWTH

SYMPOSIUM ARRANGED BY
THE POPULATION ASSOCIATION OF AMERICA

HELD AT

THE AMERICAN PHILOSOPHICAL SOCIETY

NOVEMBER 18, 1938

COST ANALYSIS OF SCHOLARLY PERIODICAL PRINTING

Preliminary Report of the Committee on Abstracting and Documentation of Scientific Literature of the National Research Council

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Professor of Botany, University of Pennsylvania

(*Read February 18, 1938*)

THE publication of scientific research, in periodical form at least, is in America very largely managed by scholarly institutions or organizations. In this respect it contrasts with much of European periodical publication, which in large part is owned and managed by publishing firms. We seem here to prefer that investigators retain control of the publication of their efforts. While we are not concerned here with the advantages or disadvantages of these two systems, it is important to call attention to certain consequences of the American procedure.

Editors and managers (often combined in the same person) of research publications are usually appointed by boards or councils or may be elected by popular vote. Prominence as investigators, managerial ability, popularity, etc., very properly and normally enter into the selections. But of one thing we may be sure—knowledge of even the elements of printing economy is probably almost never a consideration in the choice. It is not argued that it should be, but the fact needs emphasis in considering the problem to which the Committee has addressed itself.

Moreover, editors and managers nearly always serve without compensation. The institutions to which they are attached are usually quite willing that some time be devoted to editorial or managerial work, and more often than not feel honored in that members of their staffs have been se-

lected for the posts. And so printing costs constitute essentially the whole financial problem of most research periodicals. But under this system there is likely to be a rather rapid turnover in editors and managers. One comes to feel that he has done his bit and that it is time for someone else to do his. Consequently, by the time a managing editor has become acquainted somewhat with printing, and possibly with its economic aspects, he is likely to pass on the problem to another who again starts from the beginning.

Confronted by a printing problem about which he knows little or nothing, the new incumbent usually continues the periodical in the same physical form though this may be highly uneconomical. Suggestions from the printers in the interest of economy are more often than not rejected because they would involve a break with tradition, because of doubt that they are of real consequence, or even because of disinterest in all but the scholarly aspects of publication. If printers would consent to be brutally frank, many of them would tell us of their surprise that scientists can on occasion be so unscientific and even unreasonable in their reaction to sound common sense suggestions. In spite of these difficulties, printers have done much to help scholarly publication. But having done what they reasonably can, they naturally try to accommodate themselves to the editors' wishes and give them what they want.

In the last dozen or fifteen years, however, the problem of financing research publication printing has become notably more critical. Funds for printing the mounting volume of research output became more and more inadequate. Delays in publishing meritorious manuscripts became more aggravating. Pressure increased on organizations for funds to help defray printing costs. As a result, supplementary funds in aid, reaching at least several hundred thousand dollars for the natural sciences alone, have within the past decade and a half been granted to research periodicals to help pay printers' bills. In the main these subsidies have been purely in the nature of palliatives. With

their disappearance periodicals found themselves in as bad condition as before—in some cases worse.

One fact is emerging fairly clearly from this general situation. Printer and editor are appreciating increasingly that their interests and welfare are essentially mutual. With a periodical in financial straits, with growing delays in publishing accepted manuscripts, the more outstanding papers seek publication elsewhere. In consequence the scholarly standard of the periodical falls, as does financial support through membership dues and subscriptions. As a result, there is less money for printing. The picture is one of a downward spiral, with printer and investigator fellow sufferers. On the other hand, with research periodicals in economic health (which means sufficient funds to pay for necessary printing) the picture is reversed and printer and scholar both share in the benefits.

Printer and editor are therefore increasingly learning that co-operation looking toward greater economic soundness of research publications is a matter of intelligent self interest. If, therefore, sound, practical, and fundamental principles can be developed capable of making substantial contributions to dignified economy, conditions are fairly favorable for their gradual adoption through co-operation of printers and editors or managers. What seems to be called for is a scientific study to determine whether such principles can be found, and, if so, to develop them in a form at once so simple and convincing that their application in practice is likely to follow. The efforts of the Committee have been directed to this general end. To date the study has been limited to the conventional method of printing, *i.e.,* from raised type.

I. Printing Operations and Their Cost Behavior

It will be desirable at the outset to set forth the major operations in printing a journal, and to trace the cost behavior of each, in order that the principles which emerged from the study may be clear.

Printing operations may be logically divided into two classes:

A. Composition and proof-reading;
B. All subsequent steps.

As the cost behavior of these two classes of operations is, with very minor exceptions, fundamentally different, and as some of the steps are scarcely or not at all known to most editors, it will be profitable to describe them briefly.

A. Composition and proof-reading

Under this class we are concerned with operations the cost of which is for all practical purposes quite independent of the number of pages over which a given amount of manuscript is finally spread.

(1) *Composition.* Committing manuscript to type in scientific journals is, with minor exceptions, machine work. Whether monotype or linotype, the initial operation is similar to typewriting. Whether the type is 8, 9, 10, 11 or 12 point (smaller or larger type is not much used in text), whether the lines are leaded, *i.e.,* whether or not spaces appear between the lines, whether the lines are long or short, it will cost about the same to set a given number of words in type. In other words, *the cost of composition of a given manuscript, whatever the format or typography, will be determined almost entirely by the number of words to be set* (or, more precisely, by the number of key strokes necessary to set the words) *and scarcely at all by the number of pages over which it is eventually spread.*

(2) *Proof-reading.* The cost behaviour of proof-reading in the print shop (it is not generally appreciated that the excellence of most proofs of scientific journals coming from our recognized printers is due to extensive and critical proof-reading by the press) evidently follows closely the pattern of that of composition; *i.e., the cost of proof-reading a given manuscript, whatever the format, depends upon*

the number and nature of the words to be read and not at all upon the number of pages over which it is finally spread.

B. Operations subsequent to composition and proof-reading

Under this class are encountered operations the cost behavior of which is in striking contrast to those in Class A. With minor exceptions, the costs vary directly with the number of pages, and are quite unaffected (directly) by the number of words per page.

(1) *Make-up.* In this operation pages are made up out of the type which up to this point has been in galley form. Besides getting exactly the right amount of type or other material on the page and in the proper sequence and with correct spacing (except spacing in the regular text matter, which is cast with the type), the process involves insertion of running heads and folio numbers. The cost of this operation is essentially independent of the number of words on the page; *i.e.,* it costs about the same to make up a page containing 500 words as it does to make up one containing 250 words. Thus, for a 10,000 word manuscript, make-up will cost about twice as much if spread over forty pages as if spread over only twenty.

(2) *Imposing.* In this step the made up pages are so arranged in "forms" or groups (4, 8, 16, 32 pages) that in the printed and folded sheets the pages follow in proper numerical sequence. Obviously, the cost behavior here is as in make-up, imposing for a manuscript of 10,000 words again costing twice as much if it occupies forty pages as if it occupies only twenty.

(3) *Lock-up.* This is an exacting process in which the imposed pages are spaced with great accuracy, not only to insure uniform and correct margins but also that lines of type on opposite sides of the printed sheet will so far as possible "register," *i.e.*, be exactly opposite and thus avoid showing through the sheet. This accomplished, the spaced and imposed pages are "locked up" in steel chases, *i.e.*, wedged in so securely that the "forms" can be handled

freely without loss of type or disturbance of the spaced pages. In this condition the type is taken to the press. It will be apparent at once that here also cost behavior is as in the two preceding operations. Again, "lock-up" for a given manuscript will cost twice as much if it is spread over twice as many pages.

(4) *Make-ready.* When the locked-up "forms" are put on the press and a "press proof" is made, it becomes apparent that the type, cuts, tables, and other material within the individual pages of the "form" do not present a flat, even-printing surface; there are gentle hills and valleys. (These press proofs resemble cheap printing in which some areas on the pages appear too black, with the type gouging into the paper, other areas too pale.) To produce satisfactory even-appearing printing, these inequalities must be compensated in some manner, the operation accomplishing this being known as "make-ready" (for printing).

It is not practical to raise the type in the valleys to the level of the type in the hills, nor, conversely, to lower the type in the hills to the level of the type in the valleys. What is done, therefore, is to build up hills on the cylinder which carries the paper and brings it in contact with the type in the actual printing operation. These hills correspond exactly to the valleys in the type, and are opposite them; this is feasible because a given area on the cylinder (and therefore of the paper) always strikes the type over exactly the same area. In the subsequent printing, therefore, what actually takes place is that two surfaces, both uneven in themselves, come in contact, namely the type and the paper. But since the two surfaces represent a positive and a negative, with the hills of one fitting exactly into the valleys of the other, the printing pressure is sufficiently even to produce a uniformly printed sheet.

Make-ready is an operation requiring considerable skill, and much depends on it in securing a good finished product. In high grade printing by conventional methods it is still

entirely a hand process, all mechanical make-ready devices thus far having proved unsatisfactory. Successively smaller and smaller pieces of paper are cut out by hand, each shaped like the valley in the type as indicated by the press proof, and these are pasted in the same order in exactly the right position on the cylinder. Thus there is built up on the cylinder a hill the peak of which fits into the deepest part of the type valley, and the sloping sides of which correspond with the rising sides of the type valley. Usually a number of such "build-ups" are necessary for each type page.

Make-ready is an expensive operation, not only because the work requires time and skill but also because the costly press must meanwhile necessarily remain idle. And yet few editors are aware that there is such a process. This is important because here again we are concerned with an operation the cost of which is determined almost wholly by the number of pages. In other words, make-ready on a 10,000 word manuscript will cost about half as much if accommodated on twenty pages as when spread over forty, as the number of words on a page does not affect make-ready costs significantly.

With make-ready completed, actual printing can at last begin. It is important to note at this point that all the foregoing costs, including make-ready, have been incurred before there is a single printed copy in hand. Of these costs, by far the largest part is due to composition and the attendant proof-reading—both determined by the number of words and not by the number of pages. But a substantial fraction is assignable to the operations beginning with make-up and ending with make-ready, the costs of all of which are determined by the number of pages and not by the number of words.

(5) *Press work* (text). Printing of scientific journals is done in "forms," *i.e.*, in groups of pages locked up in a chase. The number of pages in a "form" is commonly 16 or 32; as sheets are printed on both sides and cut in halves,

this gives rise to the conventional 16- or 32-page "signatures." As the number of pages in an issue may not be an exact multiple of 16, a few smaller forms (2 or 4 pages) may occasionally have to be printed. If 4-, 8-, and 16-page forms are all printed on the same press, then the presswork for 4 pages will cost approximately as much as for 16 pages. But the printer makes as many 16- (or 32-) page forms as possible and resorts to smaller forms only to provide for the incomplete multiple. (What size standard forms a given printer regularly uses will depend upon his equipment—size of presses and folders.) It follows, therefore, that, with these minor exceptions, the cost of press work for a manuscript or issue of a journal, in a given edition, varies directly with the number of pages and is entirely unaffected by the number of words on the page; press work on a manuscript of 10,000 words spread over 16 pages will cost only half as much as when spread over 32 pages, the edition and page size being the same.

(6) *Text paper.* Cost of the paper in a given journal in a given edition obviously varies directly with the number and size of pages, being totally unaffected by the number of words on the page.

(7) *Folding.* The printed (on both sides) sheets are machine-folded into "signatures." Cost behavior in this process is essentially as in press work, *i.e.,* total cost for a given edition, page size being constant, varies directly with the number of pages, and is entirely independent of what, if anything, is printed on these pages.

(8) *Gathering* (of signatures). Preparatory to binding, the folded signatures are "gathered," *i.e.,* arranged in proper sequence. The cost in a given edition obviously varies directly with the number of signatures to be gathered, page size being constant. But since the number of signatures depends primarily upon the number of pages, the cost of gathering again varies approximately directly with the number of pages; and again it is a matter of complete indifference whether or not any print appears on the pages.

(9) *Binding:*

(*a*) *Wire stitching.* Depending upon the number of pages in an issue, saddle stitching (for smaller issues) or side stitching (for larger issues) is employed. By either method, stitching a copy is a single operation, the cost of which is essentially unaffected by number of pages (and, of course, by the print on the pages). Wire-stitched binding costs thus behave differently from all foregoing costs in that they are essentially unaffected by either number of pages or the number of words on the page.

(*b*) *Sewing.* For most periodicals by far the better and most used form of binding is sewing by signatures. This requires individual handling of each signature in each copy. The cost, for a given edition, thus varies approximately directly with the number of signatures, and, therefore, with the number of pages, essentially as in gathering.

(10) *Cover paper and covering.* Every copy of a periodical usually has a 4-page cover. Whether the issue has many or few text pages and whatever may be on the text pages, the cover will be the same except for trifling differences in the amount of paper for cover; also, for a given edition, the labor of affixing the covers will be much the same whatever the number of pages in the issue. The cost behavior is therefore as in wire-stitched binding—essentially independent of the number of text pages or their content.

(11) *Addressing, wrapping, and mailing* (exclusive of postage). For most periodicals these operations are performed by the printer. Obviously, the cost will not be appreciably affected by number of pages in the issue, and not at all by the content of the pages: each copy sent out requires a wrapper, addressing, and mailing. Cost behavior of this operation is thus essentially as in wire-stitched binding and in cover-and-covering.

(12) *Postage.* As most scientific periodicals appear at least four times a year, domestic copies are mailed at sec-

ond class (pound) rates. With a given number of copies, printed on a given weight paper, postage costs will therefore vary essentially directly with the number of pages, paper page size being constant.

The cost behavior in the foregoing operations and items may be summarized by arranging them in three categories; following each are listed the operations with corresponding cost behavior:

A. Costs determined essentially by the number of words: (1) Composition; (2) proof-reading.

B. Costs determined essentially by the number of pages: (1) make-up; (2) imposing; (3) lock-up; (4) make-ready; (5) text press work; (6) text paper; (7) folding; (8) gathering; (9) binding by sewing; (10) postage.

C. Costs determined neither by the number of words nor by the number of pages: (1) binding by wire-stitching; (2) cover paper and covering; (3) addressing, wrapping, and mailing.

From this summary certain general conclusions emerge pointing the way in searching for opportunities to effect economies in printing through other than editorial means (with which this study is not concerned):

(1) There is no opportunity for appreciable economies in *A* (composition and proof-reading) once the editor has decided on the final form of a manuscript, including the typography required and the format.

(2) So long as even the cheapest form of binding is required (wire stitching), and so long as the periodical is to have a cover and is to be mailed in an addressed wrapper, the opportunity for economies in category *C* is limited practically to the use of cheaper cover paper, cheaper wrappers, and possibly a cheaper type of addressing device (stencil).

(3) Since the cost of the ten operations or items in category *B* is in every case exactly or approximately proportional to the number of pages, there is opportunity for very

substantial and fundamental economies if by increasing the amount of manuscript accommodated on a type page (through typographic and format adjustments) the total number of pages is sharply reduced.

II. The Cost Analysis

A. General

Inquiry disclosed that in checking and comparing printing prices, editors think almost wholly in terms of price per page without considering the number of printed words delivered per page at a given page price. If an editor finds that his journal is printed at a lower price per page in comparison with other journals, this is usually accepted as sufficient evidence that his journal is being printed at a very favorable price. This conclusion is often reached without knowing the edition of the journals with which the comparison is made.

How meaningless, misleading, and mischievous such conclusions, based on unadjusted price per page, may be is at once apparent in the light of the cost behavior of the various printing operations described above. Before a per page price can be a valid and reliable basis for comparing costs in different journals, it must be qualified and adjusted in many ways, including words per page, edition, etc. But by the time this is done the unit has become complicated and difficult of interpretation and application, especially by editors, who, by and large, are uninformed in such matters. It seemed, therefore, that a better unit must be found, one that requires little or no adjustment and which is easily understood even by the uninitiated. We will do well to get rid of so deceptive a comparative unit as unadjusted price per page.

After some preliminary experimentation, there was chosen as a comparative unit the *cost per word per 1000 copies.* This unit will require explanation and justification.

To compensate for possible differences in the average

length of words in different journals and subjects, the cost of an "em" of type (a unit much employed by printers for composition) per 1000 copies suggests itself. As a still more accurate unit, the cost per key-stroke per 1000 copies has been urged. The Committee hesitates, however, to adopt either, though the key-stroke analysis should be made as a final test of the word unit. Our hesitation grows out of the following considerations. Both "em" and "key-stroke" are rather technical units. As we believe our task is primarily to educate editors and managers, technical units which are foreign to the editors' ken should, so far as possible, be avoided. On the other hand, the unit "word" is familiar to everyone: indeed, editors think in terms of words in judging the length of the text of a manuscript.

To test the validity of the unit selected, there were made three random samplings (to determine word length in letters) in each of three of the twelve journals studied. In each sampling the number of letters was counted in one thousand consecutive words. The data are as follows: 4.933, 4.779, 5.181—average 4.964; 4.968, 5.015, 5.246—average 5.076; 4.756, 5.002, 5.108—average 4.955. The percentage difference between the extreme averages is only 2.44.*

The edition of 1000 was selected because 1000 is probably not far from the average edition of scholarly periodicals, at least those in the biological sciences.

It was then decided to attempt, on the basis of the above unit, an accurate comparative cost analysis of a limited number of representative research journals. And, finally, to determine what, if any, correlations of the results exist, especially with such factors as average number of words per page. The twelve journals analyzed to date were all chosen from the biological field to avoid possible differences

* Since the manuscript went to press, the soundness of the unit selected has been further tested by determining the cost per key-stroke per 1000 copies in three of the twelve journals studied. The results, to be published in detail elsewhere, are in complete agreement with those obtained for the cost per word per 1000 copies.

in average word length in different sciences. All were standard research journals. The selection was made wholly without reference to printers. Editors were invited to co-operate by furnishing: (1) copy of the printer's bill for a recent issue; (2) copy of the journal issue corresponding to the bill; (3) copy of the printer's bid (if any) under which the journal was being printed. All issues of journals analyzed fall within the period January–June, 1937, and are therefore strictly contemporaneous.

As the unit of comparison selected is cost per word per 1000 copies, all those parts of journals which cannot be reduced to this unit were excluded, as follows:

(*a*) Covers and advertisements (largely standing matter).
(*b*) Cuts and illustrations.
(*c*) Tables. There appears to be no valid unit for comparing tables: area, words, figures, ems, key strokes, etc., all are relatively meaningless.
(*d*) Special formula matter. As such copy is set slowly and occurs only in some journals, it was considered fairer to exclude it. The amount encountered was negligible.
(*e*) Blank pages (if differentiated in cost from the lowest-cost composition).

The scientific journal printers consulted all agreed that it was wise and fair to exclude the above items.

For the text matter all print shop charges were included except wrapping, mailing, and postage. These were excluded because the corresponding costs were not available in all cases (due in some instances to the fact that the printer does not attend to the mailing). It is advisable in any event to exclude postage because of the difficulty of adjusting the cost to 1000 copies, especially when it is not known how to apportion the copies mailed at foreign and at domestic rates. Also, postage is not properly a printer's charge; he merely collects from the editor what he has advanced for the purpose.

B. Detailed procedure

(1) *Allocation of text matter to the categories of charges in the bill.* All text matter in each issue analyzed was accurately measured. Each type of text composition (8 pt. solid, 8 pt. leaded, 10 pt. solid, etc., etc.) was separately measured and allocated to its corresponding charge category appearing in the bill. The totals (in pages) so arrived at and the totals in the printer's bill should agree. In case of any disagreement, the source of the discrepancy was located, sometimes through correspondence with the printer. Not until this agreement was satisfactory did the analysis proceed.

Only after one knows how all material is allocated by the printer to the composition categories can one be certain what is to be considered text and therefore to be "word-counted" (see below). To illustrate: tables usually have a heading, and often footnotes; in fact, these accessories sometimes occupy more space than the strictly tabular part itself. How do printers interpret, and therefore charge for, these three parts? In some cases only the strictly tabular part is charged as tabular; thus the heading and footnotes are assigned to text, in whatever category or categories the type size indicates. In other cases the heading and strictly tabular part are both allocated to tabular and charged accordingly, leaving only the footnotes as text in the appropriate type-size category. Finally, all three parts are in some cases allocated to the tabular category and charged accordingly, leaving nothing assignable to, and countable as, text. Incidentally, this example illustrates the uncertainties attending the interpretation of competitive bids in the absence of rigid specifications.

(2) *Word count of text.* To determine the total number of text words in each issue analyzed, the sampling method suggests itself, i.e., counting the words on a number of pages chosen at random. On a priori grounds of doubtful reliability, the method was soon shown to be in fact most

unreliable, due in part to the fact that most technical scientific journals are so unequally spotted with tables, cuts, headings, etc. The method exclusively used, therefore, was complete counting of every text word in each issue analyzed. Nearly all the counting was done by one person whose accuracy, as shown by scores of random checkings, proved to be uncanny. For types of material presenting counting difficulties, e.g., bibliographies, more or less arbitrary methods were adopted and applied uniformly in all counting. For instance, the initial (with its period) of an author's name and the volume number with the following colon were each considered a word and so interpreted in all counts.

(3) *Average number of text words per page.* With the total number of text words known from the count, and with the number of pages devoted to these text words known from the printer's bill, the average number of text words per page was calculated for each issue analyzed.

(4) *Cost per text word per 1000 printed copies.* With the total number of text words in the issue known, with the number of pages devoted to these text words known, as well as the total charges for these pages, adjusted to an edition of 1000 copies, the cost per text word per 1000 copies was calculated. (The cost in any other edition can be easily ascertained.) Text paper is included in this cost. At first it was planned to exclude it because of possible distorting effects in those instances in which editors happen to insist on expensive or very heavy paper. But the fears were ungrounded in the twelve journals analyzed. No great paper price variation was found, the most costly being a 30 per cent rag book paper. As the edition of 1000 is small and the paper consumption very limited, the differences in the per word costs occasioned by differences in pound price of the paper were so small that they were ignored. Here again the printers consulted agreed that the decision was sound.

(5) *Correlation of cost per word per 1000 copies with*

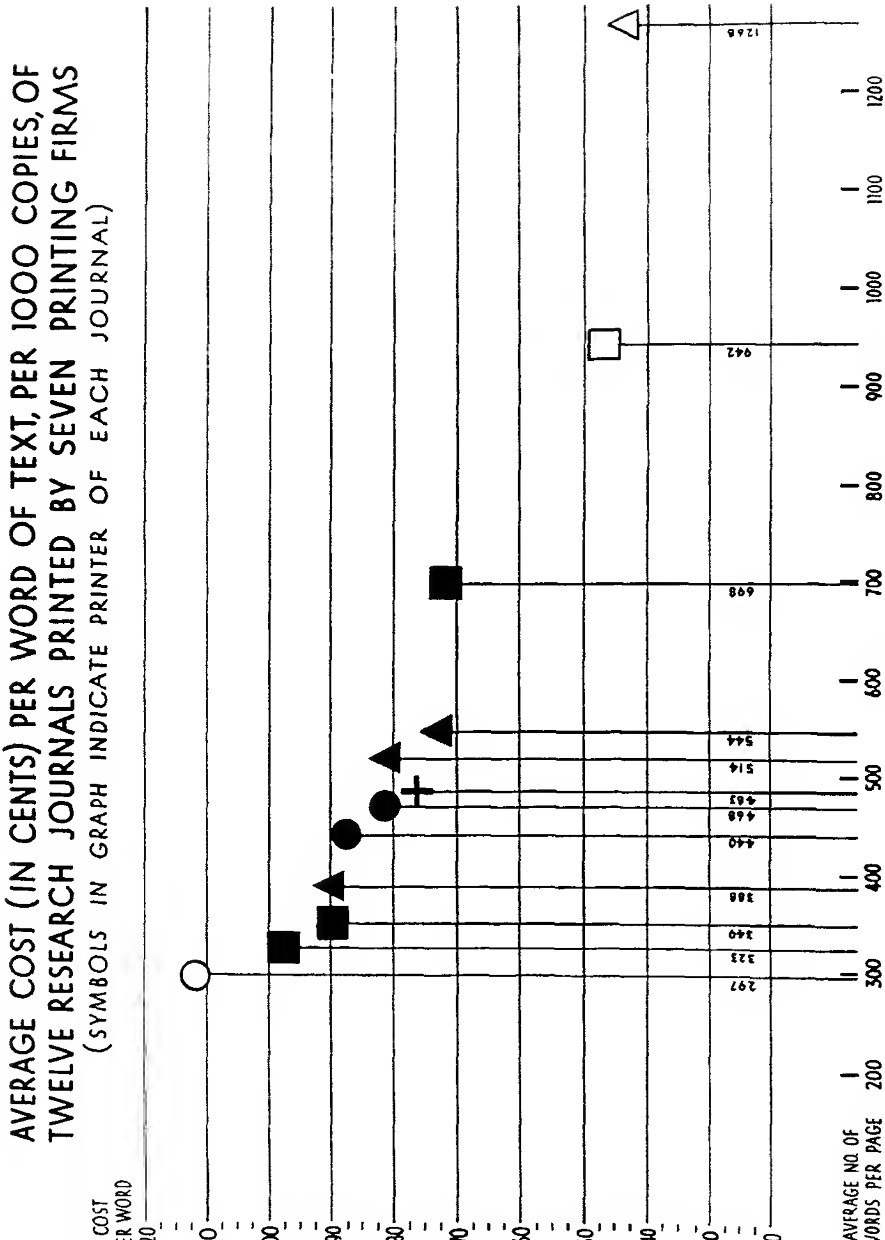
AVERAGE COST (IN CENTS) PER WORD OF TEXT, PER 1000 COPIES, OF
TWELVE RESEARCH JOURNALS PRINTED BY SEVEN PRINTING FIRMS
(SYMBOLS IN GRAPH INDICATE PRINTER OF EACH JOURNAL)
COST PER WORD
120
110
100
0.90
0.80
0.70
0.60
0.50
0.40
0.30
0.20
AVERAGE NO. OF WORDS PER PAGE
200
300
400
500
600
700
800
900
1000
1100
1200
297
323
340
388
440
468
483
514
544
698
942
1248

Fig. 1.

the average number of text words per page. The cost behavior of the various printing plant operations described earlier is such that the existence of a general negative correlation is confidently to be expected. A curve was therefore plotted with the cost per word per 1000 copies represented on the vertical axis, and the average number of text words per page on the horizontal axis. The result is presented in the accompanying figure.

C. Interpretation of the results, and conclusions

It has sometimes been suggested that a better way to investigate differences in cost due to format and typography, especially as these affect word content per page, is to invite printers to bid on the same manuscript printed in several different formats (with whatever accompanying typographic differences seem advisable). It is argued, and correctly so, that in this way the material is absolutely identical. But the Committee is convinced that this method is less reliable than the one chosen, and for the following reasons: (*a*) It at once places the matter on a semi-theoretical basis, whereas the method used reflects the state of affairs just as it exists and as it has naturally developed in actual competitive commercial practice. (*b*) In those instances known to us in which printers have been asked to indicate whether savings could be effected in journals of small edition by changes in format and typography resulting in more words per page, the printers have answered in the negative. Where editors subsequently secured competitive bids on a new economical format and typography, the printers invariably concluded that substantial savings were after all possible.

Special attention is called to certain facts revealed by the graph:

(1) The journal with the lowest average number of words per page is paying the highest price per word for printing.

(2) The journal with the highest average number of words per page is paying the lowest price per word for printing.

(3) In the journals which fall between these extremes in average number of words per page, the price paid per word for printing drops consistently, with only minor irregularities, as the average number of words per page increases.

Since the figure does not reveal the following facts, which are however contained in the printers' bills or correspondence with the editors, attention is called to them here: (*a*) The price charged *per page* of text was *lowest* in the 297-words-per-page journal and *highest* in the 1268-words-per-page journal. From competitive bids and comparisons of *cost per page* with other journals, the management of the former journal concluded that the printing was on an economical basis. This incident illustrates how meaningless and misleading is an unadjusted price per page as a comparative yard stick. Using it, the management was lulled into a spurious feeling of security; actually it is paying the highest price for printing of any of the twelve journals thus far analyzed.

(4) The spread in cost per word from the 297-word- to the 1268-word journal is from 1.12 cents to 0.45 cents per word per 1000 copies, the latter about 40 per cent of the former.

(5) For any given printer, the cost per word per 1000 copies in every case decreases with increase in the average number of words per page. This is important since personnel, management, wage and salary scales, accounting system, overhead charges, etc., are presumably the same for the journals printed in the same shop.

(6) Irregularities occur in the curve. Since efficiency, wage and salary scales, and other factors vary more or less from printer to printer, it is surprising that the curve is not more erratic. It is fully expected that as more journals are analyzed, further irregularities, doubtless more pronounced ones, will appear. It may well be that with

a basically sound comparative yard stick, as the one used is believed to be, these irregularities will in some measure reflect differences between printers in either efficiency, wage scale, overhead charges, profit basis, or other factors, or combinations of these.

(7) The reduction in printing cost resulting from accommodating more words per page (which is another way of expressing reduction in number of pages) is sharp and very substantial even in the comparatively low edition of 1000. This is a very important point, because it demonstrates that opportunities for significant reduction in costs are available in the very edition ranges characteristic of a great many scientific and other scholarly journals. In journals of larger editions the savings will be proportionately larger because the printing operations involved in producing larger editions are all such as show a direct relation of cost to number of pages.

It is almost the universal reaction of editors, and, be it said, of some printers, that no worthwhile economies can be effected by reducing the number of pages (by accommodating more words per page) because the only savings would be in paper and press work—savings which in these small editions would not be really appreciable. It is true that no savings are effected in composition and proofreading by reducing the number of pages. But in addition to press work and paper, all of the following printing operations are likewise reduced in direct ratio as the number of pages is reduced: make-up, imposing, lock-up, make-ready, folding, gathering, and binding (by sewing). The reduction in cost in all these items combined is responsible for the remarkable effect, even in editions as low as 1000, of increasing the number of words per page and thereby reducing the total number of pages. (As will be shown later, additional substantial savings are contributed through the skillful handling of tables, cuts, etc., in a highly economical format.)

Once a journal has adopted a definite format and typog-

raphy which fix it at a certain average-number-of-words-per-page point on the horizontal axis, the opportunities for achieving economies through competitive bidding by printers have been confined within relatively narrow limits. There is little use in an editor shopping about in an endeavor to get into the price range of a 700-words-per-page journal while insisting upon a format and a typography which keep his journal in the 300-words-per-page class.

(8) As a corollary of the preceding, it follows that the realization of substantial economies in scholarly publication by conventional printing methods is dependent primarily upon decisions resting with editors and managers.

III. Means of Increasing the Number of Words per Page

Since reduction in the total number of pages is the most effective way of achieving substantial economies, the practical problem arises of how to step up the average number of words per page. Obviously this can be done in a number of ways and combinations of ways. What method or methods are used will depend on how drastic a change is undertaken; feasible methods can be devised which will place a journal almost anywhere on the horizontal axis of the figure without exceeding acceptable octavo sizes. We are therefore not confronted with an all-or-none situation; on the contrary, we have wide latitude of action.

A. *In one-column format*

Among methods available for increasing the number of words per page while maintaining the conventional one-column format, the following may be mentioned:

(1) *Lengthening lines* by reducing margins or increasing page size slightly, or by a combination of the two. There are fairly sharp practical limits to these methods. Also, as the lines are lengthened, reading difficulties begin to appear, the correction for which is leading (spacing be-

tween the lines). Leading, however, tends to nullify the gains achieved through lengthening of lines.

(2) *Decreasing the type size.* If a large type size (13 or 12 point) has been used, this procedure has possibilities, although again not within wide limits because reading difficulties soon appear.

(3) *Substituting a more compact (condensed) type* in case a wide (expanded) type has been used. The gains, while appreciable, are sharply limited owing to the emergence of reading difficulties.

(4) *Reducing or eliminating leading.* What can be gained here will depend on how much leading can be removed before serious reading difficulties arise; the limits are obviously sharp.

(5) *Various combinations of the above methods.* The Committee has made no real study of the possibilities of increasing the average number of words per page in one-column format while maintaining satisfactory readability and over-all size not exceeding a convenient octavo. But to hazard a guess based on such experience as we have, the limit that can be attained by these methods under these conditions is probably around 550 words per page.

B. In two-column format

The limitations that arise in applying the methods discussed under one-column format are due primarily to resultant reading difficulties in the long lines. These limitations are very substantially reduced, in some cases eliminated, if the long lines of one-column format are broken, *i.e.*, if a two-column format is substituted. Within limits the columns can be made any width desired. However, there are solid reasons in favor of a column about three inches wide. Much narrower columns should be avoided because technical difficulties arise which result in waste of space; it is better to enlarge the over-all size slightly if necessary in order that columns can be used of sufficient width to offer the maximum in practical economy.

Some of the advantages gained by a suitable double-column format are as follows:

(1) Type size can be reduced to 9 point, or even 8 point, while retaining excellent readability.

(2) Leading can certainly be reduced to 1 point, or in some cases eliminated entirely.

(3) A more compact (condensed) type can be used than in longer lines, without seriously interfering with readability.

With an increase in over-all dimensions of not over one-half inch in height and two-thirds inch in width, using 9 point type leaded 1 point in two 3-inch columns, the average number of words per page in our octavo one-column scientific journals can in most cases be at least doubled and in some cases trebled, with attendant drastic reduction in total pages and therefore in printing costs.

Among other advantages of the double-column format the following should be mentioned:

(1) *More rapid reading.*

(2) *Economy in tabular material.* Tabular material is charged for by the page, the rate usually being the highest of all types of composition. In one-column format all tables interrupt the type across the entire page. In two-column format many of the narrower tables are readily accommodated in one column; such tables thus occupy only half as many pages, resulting in reduced cost of tabular matter as well as increased room for words, thereby effecting a further reduction in pages.

(3) *Economy in cuts and text illustrations.* As in tabular matter, cuts and text illustrations interrupt the type across the entire page in one-column format (adapting type around cuts is not economical in journals of small edition). But a very large number of such cuts and illustrations, either without reduction or with permissible reduction, are admirably accommodated in one column of a two-column

format. The space saved (and it is substantial) is available for more words, with a corresponding reduction in total number of pages. Furthermore, there is a saving in making those cuts which are made smaller in order to fit them into one column of the two-column format. And if the cut maker is instructed to make all such *blocks* (not printing surfaces) exactly the width of the column, much time is saved in make-up which, if called to the bidder's attention, will probably find reflection in somewhat lower rates.

(4) *Space,* and therefore pages, *saved in column headings* of all sorts; in a one-column format these break across the entire page.

(5) *Space,* and therefore pages, *saved in* (*a*) *footnotes,* (*b*) *bibliographies,* (*c*) *the end of paragraphs,* (*d*) *legends,* etc., because the blank portions of incomplete lines are very much shorter.

Thus there are opportunities for substantial economies by adjustments in format and typography. How far editors wish to go, and whether they will venture across the Styx into the two-column nether regions, rests with them. Clearly, the greatest opportunities for very substantial savings, while retaining admirable readability, are trans- rather than cis-Styx.

IV. Specifications for Obtaining Printers' Bids

This is a difficult subject and the Committee has not, during the present study, given it more than casual attention. However, we have obtained a fair cross-section of the situation by seeing such specifications as editors draw up, if indeed any are submitted to printers. We have also seen numerous printers' bids and contracts and how these are applied and interpreted in bills for finished work. So while no analysis of the subject is attempted at this time, attention is called to certain facts.

By and large the specifications which we have seen are

very unspecific in many directions. Much is therefore left to the interpretation of the individual printers. The bidding presses, however conscientious, are therefore not shooting at the same mark. The result is that the editor can not know who in reality is the low or the high bidder because he does not know just what each press will deliver for a certain charge, and this is through no fault of the presses. But if perchance there should be an unscrupulous individual submitting a bid, such a bidder would have a large advantage because the vagueness of the specifications would entitle him to apply the quotations to actual work in such a way that what seemed to be a low bid turns out to be a high one.

The point here is not that recognized presses would stoop to sharp practices: the standards in these are high. But clear specifications, including typographic requirements, make it possible for printers to submit sounder bids, all directed toward the same target. Under these conditions there is not only greater fairness to the bidding printers but editors will be rewarded by further printing economies.

THE SCHOLAR AND THE PUBLIC

DUMAS MALONE

Director Harvard University Press

(Read February 18, 1938)

SOME days ago I received from Mr. Bean a letter in which he asked just what I expected to talk about at this meeting. He said that he himself would probably stay on the practical side. I replied that it was a comfort to me to know that the practical questions connected with scholarly publishing would be so well taken care of, and that, since I had had so much less experience, I supposed I should stay on the theoretical side. However, my ideas on this subject, such as they are, have also come as the direct result of practical experience. Mr. Bean and I stand in essentially the same position, somewhere between the scholars and the public. As publishers in general are the middlemen of literature, so academic publishers are the middlemen of learning, though it is to be hoped that literature also falls to some extent within our province. I sincerely trust that the terms "learning" and "literature" are not mutually exclusive. During the years that I was an editor in Washington, my task might have been described as the presentation of the work of scholars to the larger public. More recently, I have also had to deal, as Mr. Bean has, with the problem of distribution. So I am quite accustomed to this intermediate position, between scholars and public, and also to receiving brickbats from both directions. What I want to try to do tonight is to present a personal point of view, I hardly dare call it a philosophy, to which I have come from this experience. I hope you won't mind if I try to simplify the problem of scholarly publication, without any thought of being at all profound. In my own ef-

forts to understand the situation I have tried to find what is fundamental and essential, and thus to reduce the problem to its simplest terms.

The first factor, of course, is the scholar, who investigates something. He may not like being called a simple term. Indeed, scholars investigate almost anything, almost everything: the roots of words or the roots of wars, legal transactions in Babylonia or the baking of bread in Chicago, mediæval walls or colonial furs, diction or dictators, the rhymes of poets or the reason of philosophers, the seeds of watermelons or the seeds of culture. The Commencement program of any American university, and almost any list of scholarly projects, will reveal the extraordinary diversity and, not infrequently, the humorlessness of research. It is not my purpose to indulge in wisecracks about bizarre and esoteric titles, but merely to point out that the freedom of investigation is practically unlimited.

A scholar may inquire into anything he likes, if he has the time and the facilities. Except when he seeks a leave of absence or financial aid for a particular enterprise, he need consult no one. He may act on reasons that seem good to him, without regard to the opinions of anybody else. Generally, he is affected by the ideas which chance to be prevalent in his particular chapter of the scholastic guild, and he is not necessarily unaware of other opinion, but, nonetheless, few people enjoy such great and glorious freedom as he. In this country, no serious effort has been made or is being made to limit, direct, or control his activity. His own curiosity and intelligence and conscience are his guides.

Sooner or later, however, he becomes aware of the fact that no investigation is complete unless, somehow, its results are recorded. The inchoate mass of notes in the filing cabinet must be given form; the discoveries of the inquiring mind must be shared with others. The question inevitably arises whether or not there is freedom of publication comparable with that of investigation. At this moment the

shades of the prison house begin to close. The scholar who has convinced himself of the desirability of his investigations must now convince somebody else of the value of the results. It is true that almost anything can be privately printed at the author's expense, but the name of no learned society or publishing house can appear without previous agreement. Publication is contingent on consent.

In the consideration of a prospective book, two fundamental questions must be asked: Should it be published? Can it be published? That it *should* be published, the author does not doubt, and few of his professional colleagues like to take the responsibility of telling him it shouldn't. Even in our palmiest days, it was never true that *all* the results of research were published, or that they seemed equally important. The entire crop of doctoral dissertations has not been exposed to general view. Many people regard this as fortunate. Nonetheless, the general opinion has been that anything that a reputable scholar regards as significant deserves publication, especially if he has worked on it for a long time. No one, I believe, has gone so far as to say that all facts are equally important, but most of us have been reluctant to describe the findings of someone else as trivial, that is, previous to publication. Perhaps it is accurate to say that scholarly writings in general have been subjected to relatively little criticism in advance. The common opinion seems to have been that, if possible, the individualism of investigation should be carried the next logical step: as scholars might explore anything, so they might publish anything, provided that it had a moderate degree of technical correctness and that means could be found. Also, in those good old days, it was practically assumed by all of us that, somehow, means would be found.

It is hardly necessary to remark that in these parlous times we are less optimistic. We may once have thought that both natural and financial resources were unlimited in this most fortunate of lands, but surely we can cherish that illusion no longer. We are faced with the necessity,

as we so often are in our own families, of choosing between expenditures, all more or less desirable in themselves. What is spent on one thing cannot be spent on something else. Also, the scholars are beginning to become faintly aware of the problem of distribution, and to realize that production and distribution may have been out of balance. For financial reasons, if for no other, we are now forced to practice more rigorous selection, whatever the future prospect may be.

I am of the opinion, furthermore, that more rigorous criticism is desirable on other than financial grounds. It may be that there is no immediate likelihood that the efforts and energies of American men of learning will be exhausted, but certainly they ought to be directed with all possible wisdom. We have assumed that there was danger in condemning any research project in advance, because no one could know whither it would lead, and that wastefulness was as inevitable in scholarship as in Nature. It is my personal opinion that much wastefulness could have been avoided by more careful advance scrutiny. But even if it could not have been avoided and cannot be now, the situation is quite different when publication is being considered. By that time the results can be inspected; somebody can decide whether or not the investigator has gone anywhere or brought back with him anything that will be prized by anybody else. There are dangers in adverse judgments, as there are heartaches, but the responsibility for making them cannot be dodged. For the sake of scholarship itself, entirely apart from financial considerations, it seems to me that our judgment ought to be considerably more critical than it has been heretofore. The question is not merely, how should we publish, but why should we publish? Do the ends justify the means? Just what do we hope to accomplish, anyway?

The most common argument that is used to justify scholarly publication is that the boundaries of knowledge are thereby extended. The argument is still valid, though

in many fields we now seem to be in the era of exploration and settlement rather than that of discovery. Whenever we are reasonably confident that someone has surveyed important new territory, we should go to almost any amount of trouble and expense to help him describe it. But we must not mistake molehills for mountains, and we cannot afford to dignify with a book every microscopic advance of the frontier of erudition. We might well relegate the microscopic to the microfilm.

In the mind of an author and his friends, a more immediate and compelling argument is that a book will contribute to his professional advancement. On this subject I have no desire to be facetious. I am too acutely aware of the tragic human problems that are involved. In other days some of us got a ready hearing, even though we may not have had much to say. The younger group of academic scholars, in an era when appointments are harder to get and promotion is slower, will be at a serious disadvantage if they do not have as good a chance. A microfilm is distinctly inferior to a book as an advertizing medium, and printing from raised type is still more attractive than what is done by an offset process. Also, it is still true that the written work of scholars, which may be read by their elders and their peers, is a very valuable criterion of ability. But scholarly books cannot be sufficiently justified as literature of promotion, and universities would do well to devise less expensive tests of the intellectual calibre of prospective professors. Indeed, I believe that it is now incumbent upon them to do so.

Of another purpose of scholarly publishing, which concerns the public as well as the author, one hears relatively little. May not the scholar, through books, hope to reach a group not otherwise accessible to him? May not scholarly publishing serve to extend the influence as well as the boundaries of learning? If these discoveries our investigators make, if these thoughts our thinkers think, are valuable and important, they should be shared with whatever

portion of the public is capable of appreciating them. It may be a very small segment of the movie-going populace; it may be too small to encourage commercial exploitation; but that it is larger than the group of technical scholars to whom our monographs have been commonly addressed, and that it can and should be cultivated, I steadfastly believe. I know of no more effective method by which the empire of scholarship can be safeguarded and extended. Our supreme investigators and major seers may be left to their own devices, but most of us should be more mindful than we have been in the past of our teaching function. It need not be and should not be restricted to our pupils or our professional colleagues.

However you may differ with me in this, at least we can agree that the physical form in which scholarship is presented should depend upon the nature and the object of the work. Before it is decided whether to employ a learned journal, the microfilm, the offset or the printed book, or nothing at all, a fundamental question should be asked. For what reasons are these materials being presented, and to whom? The publisher inevitably thinks in terms of sales, but I wish to lay the emphasis not so much on sales as on use. The emphasis should not be even on utility in any narrow sense, but on the importance of the work in view of the problems of the world of learning as a whole. The facts which scholars discover are not equally important, and the patterns in which they arrange them vary in significance. The difficult and thankless but necessary task of committees of publication is to choose from among them and to adjust the means to the end that may be in view.

It would be hazardous to attempt to draw up a list of the categories of scholarly materials that ought to be published in books, for no one can hope to be omniscient. But anyone who is confronted day by day with the questions of acceptance and rejection, of inclusion and exclusion, has to formulate tentative principles to guide him in the fog.

So, with much diffidence, I submit certain generalizations for your consideration.

It seems clear to me that we should make printed books of doctoral dissertations and their like considerably less than in the past, even if funds were available for the inevitable subsidies. It may be that a relatively large proportion of dissertations in the physical and biological sciences ought to be made generally accessible, at least in abbreviated form. On this subject I do not presume to speak. But I feel confident that this is not true in the other quadrants of learning, the humanities and the social sciences. There will always be numerous exceptions, but in general it may be said that the purposes of scholarship can be served sufficiently in a less expensive way—less expensive in time and effort as well as money. No scholarly publishing house or committee of publication can perform its more important functions if swamped with minor monographs. We cannot afford to surrender the garden to seedlings.

The argument is often advanced that the quality of dissertations is improved by the prospect of publication, and that where there is little or no prospect of publication standards decline. I doubt if it can be proved that quality is higher in institutions where publication is mandatory, but even if it can be proved, those who advocate wholesale publication are arguing in a circle. The dissertation is not an end in itself. Our concern is for the well-being of scholarship, not for any particular mechanism.

In general, monographic studies which are chiefly valuable for their factual content but which are not likely to be used extensively as works of reference, should be reproduced in the cheapest form that is effective. The garments are of no consequence when all one wants is facts. On the other hand, form is of the essence of some works, as, let us say, studies of art. Many of these may be excessively costly and limited in appeal, but, while photographic processes may reduce expenses, it is difficult to avoid the neces-

sity of a printed book. Here my personal prejudice is in favor of curtailing the number of published studies, rather than of diminishing their effectiveness by greatly cheapening their form. Perhaps if there were fewer of them and they were more carefully selected they would be more widely read.

I feel much the same way about works in which the literary form is important. The quality of the writing may be of slight consequence in scientific, statistical, or even philological studies, provided they are intelligible; it ought to be of considerable consequence in historical, literary, and philosophical studies, to which in many cases no factual abstract can do justice. Anything which gains significance because of its method of presentation or interpretation seems more deserving of a book than does a compendium of technical information, unless the latter is a valuable work of reference. In the case of these more literary and more thoughtful studies, my prejudice is again in favor, not of wholesale reproduction in printed or in any other form, but of more rigid selection. I like the policy of choosing between the buds and giving the finest ones time to ripen. Often we pluck our flowers before they have opened and pick our fruit too green.

In my opinion, the desirability of publishing major works of scholarship in book form is unquestionable, even though the prospect of sales is relatively discouraging. The question here is of value and use rather than of actual sales. Many works that have come to be regarded as classics in their fields, that have appeared on innumerable reference lists, that have been put on the reserve shelf in libraries, that have been consulted by thousands of people, have been commercially unprofitable. Sales may be and generally are better than in the case of more restricted studies, but, in very many cases, the edition cannot be large enough to bring costs to the commercial level. If the list price is set high enough to repay costs, it is too high to attract purchasers. In order to facilitate distribution, it

is generally necessary to set the price too low to recover costs, even if the entire edition is sold. Accordingly, there is an inevitable deficit. Such a deficit, however, may be faced with relative composure in the case of an important work.

Of course there may be considerable difference of opinion in regard to the importance of any study. Obviously, mere bulk is not a sufficient criterion. A small, restricted monograph may turn out to be very significant, but the chances are in favor of books that cut across departmental lines and that seek to illuminate large areas. At the present stage of human affairs, it seems desirable to emphasize major rather than minor works. In our warfare against ignorance, we may have depended too much on rifle fire and not enough on the machine gun; we may have relied too much on isolated forays rather than on major offensives, carefully planned in advance. It is difficult for a group of individualists to work out the larger strategy of concerted action, but surely it is incumbent on every agency of publication to consider not merely what can be published but also what should be published.

Whatever difficulties and dangers may be involved in the working out of a reasoned program of publication, even in tentative form, there seems to be no other way to avoid the mechanical parceling out of grants and favors to the various departments of learning. Even though everyone may agree that from the point of view of publication the claims of departments are not equal at any particular time, any more than all facts are equally important, it is exceedingly difficult to discriminate. There may be a natural tendency, indeed, to help the weak rather than the strong, because they seem to be in greater need of help. Workers in undeveloped fields often deserve special encouragement. For example, if it be true, as I think it is, that American culture has been weakest in the sphere of music, a long-range program might well include works in musicology, regardless of popular demand. Personally, I believe the de-

mand for such books will grow. Also, because of the paralysis of scholarship in certain countries of Europe, it might be very desirable to give special attention to fields once cultivated there and now neglected. Very special obligations now rest upon us to encourage studies of certain phases of European history, even though they may have relatively small appeal to the larger American audience.

However, in planning a program, it seems to me that it would be unwise, even absurd, to put a premium on the absence or the meagerness of general interest. The probability that very few people will ever use a book is certainly an insufficient argument for publishing it. We always have a right to inquire what purpose, and how many people, a book will serve. We need not float on ephemeral eddies of taste, but

> "We must take the current when it serves,
> Or lose our ventures."

As teachers, we should certainly be foolish if we should fail to take advantage of popular interest in any field, while that interest is still strong. The fact that the public now consumes more books in economics and government than hitherto does not lessen our obligation in those directions, but increases it. We cannot claim that scholars have any monopoly on political, economic, or any other kind of truth; and we have every reason to suppose that on controversial subjects they will not be in entire agreement. From the integrity of scholarship, however, one has a right to expect significant contributions on subjects of general concern; and learning will be in grave danger of stultification if it runs away from life. It seems obvious that we should not over-emphasize the virtues of rarity. From time to time we may deliberately publish books dealing with remote fields in order that knowledge of them may not perish, but, other things being equal, I believe that preference should generally be given those subjects and those books which a relatively large number of people will use and in which

they will be interested. After all, scholarship was made for man, not man for scholarship.

Along with major works, which promise to serve important purposes, I think we should encourage books, large or small, that seek to interpret scholarship and present its fruits to a larger audience. I am not speaking of best-sellers, because these may be adequately taken care of by commercial agencies and also because, unhappily, the scholarly quality of the best-seller is likely to be low. I see no objection to our deriving any financial advantages that we can from wide sales, and no reason why scholars should be discouraged if some of their books, perchance, are read outside technical circles. With us, however, the emphasis should always be on the quality of a book, all the more if the commercial spirit has grown in publishing as a whole, as perhaps it has.

The category to which I am now referring includes works which need no subsidy and others which still need some support. I should like to emphasize the importance of such books from the point of view of scholarship, and the desirability of their securing financial aid when it may seem necessary. They must battle for recognition in a crowded and noisy world and, despite their intrinsic value and potential appeal, the odds are often against them. The cost of distributing them may be so high that they will prove unprofitable, but it is supremely important that they be distributed. Also, I should like to do something, if I can, to persuade scholars that the writing of such books does not require the prostitution of their talents. However, it does require talents which we have not encouraged and which many of us may not possess. I am afraid that it is often easier to prepare a monograph.

What I have in mind is not the cheapening of scholarship, but the interpretation and more effective presentation of it at the bar of intelligent opinion. I am not thinking of books that are less than scholarly, but of works that are more than scholarly, though they may be relatively devoid

of scaffolding and ostentatious technique. To the casual observer, anything that is well done seems easy. We need books of sound learning, illuminating in thought and interpretation, marked by positiveness and vitality, and characterized by good literary form. These may never be numerous, but surely they deserve our moral and our financial support whenever they do appear. In a real sense, they constitute the vanguard of scholarship, extending its influence and enlarging the domain of its spirit. Be it said without self-righteousness or sentimentality that they are the evangels of free, honest, and humane learning in a world wherein obscurantism, prejudice, and superstition still abound. Whether these books are regarded by the larger public as radical or conservative is a matter of small concern, for the spirit of frankness and honesty which the works of scholars should breathe is far more important than their specific content. What we do is far less important, after all, than how we do it.

As long as the human mind retains its curiosity, men will investigate; as long as there is freedom, articles, monographs, and books will emanate from all fields and corners of the world of learning. We and our successors must consider the means of presenting these, all possible means, and must adjust them to ends as best we can. At all times, therefore, we must realize that there are ends and must try to determine what they are. The scholar seems far removed from what we call the public, but he would be foolish to belittle or ignore it. Learning that is unrelated to and indifferent to life will eventually perish, either from external force or internal decay. At last, it is only by serving the public that scholarship can serve and save itself.

THE RIDDLE IN RESEARCH

DONALD BEAN

Manager, Publication Department of The University of Chicago Press

(*Read February 18, 1938*)

Nearly two hundred years ago, the society, which later changed its name to the American Philosophical Society, entered the following motion on its official minutes: "C. Thomson and Dr. Morgan are appointed to agree with the printer for 200 copies of Moses Bartram's paper on silk worms." That was a happy resolution of Mr. Bartram's publishing problem. It was probably not the earliest scientific address given before this group which was printed and distributed by the Society. I shall not regard this pilgrimage a success unless I learn more about the early publishing of this Society, and more about this particular transaction: the name of the printer for instance; the amount of his bill; and how many copies of the paper the Society still has on hand. Although the paper on silk worms is the first notation which I discovered in the minutes, I feel sure that the interest of the Society in scholarly publishing dates from its founding in 1727 and was inspired by its learned and beloved founder. No modern publisher would consider comparing his interest in scholarship or his record as a publisher with that of Benjamin Franklin. No other organization can match the continuous, constructive contribution which the Transactions, the Proceedings, and the Papers of the American Philosophical Society have made to American scholarship.

And yet, we are gathered in this historic hall tonight to discuss the present day problems of scholarly publishing, which are acute in spite of that record, and in spite of the labors of countless other societies, individuals, and institu-

tions. Some important element of the problem must have been overlooked, or this would not be so.

I propose this evening to discard most of my own previous conclusions and to approach the whole problem from a new and provocative angle. If this bold endeavor is successful, your minds will at least be diverted for a few minutes from more practical publishing details.

The attitude of the individual scholar toward his literary output is my starting point. Why do scholars attach so much importance to the prompt publication of their handiwork? In spite of our excellent record at the University of Chicago Press, nearly every scholar of the institution who is now working on a piece of productive research still worries to some extent about its eventual publication and keeps his weather eye on that problem during most of the progress of the research. I find that the same thing is true of scholars in other institutions. It is apparently their unanimous opinion that uncertainty or delay in publication discourages scholarship and research, and acts as a subtle reflection upon the significance of their work.

This attitude is even more puzzling when one realizes that most scholars have really little to worry about so far as uncertainty of publication is concerned. It was not always so, but I think most of us here tonight would agree that in 1938 little significant research will go unpublished. It may not be published promptly, to be sure, and many scholars will have difficulty in selecting one publisher for their manuscripts from the innumerable publishing outlets available to them. This complexity of the publishing machine may in itself be some clue to the cause of the scholars' anxiety. That same complexity and multiplicity may also account for most of the practical problems which we have discussed today. The good old days when the American Philosophical Society published nearly all the significant research of the country which was not published abroad by the Royal Society or the French Academy are no more.

Commercial publishers and the Smithsonian Institution were the first agencies to come to the Society's rescue. The last half of the Nineteenth Century witnessed the rise of innumerable specialized scholarly societies and their journals. Beginning about 1890 and continuing until the present time, educational institutions have vied with one another in the establishment of university presses. None of these agencies has disappeared and all of them are still busily engaged in carrying on in their accustomed manner. Many publishing problems are intensified by this situation, but I suspect that complexity is not the basis of the individual scholar's complaint.

In fact, scholars themselves are chiefly responsible for this multiplicity of publishing agencies. They established new technical societies in many cases primarily for maintaining a scholarly periodical or other publishing facilities. This was commendable and sufficient so long as these periodicals published most of the research output of the country that was not handled by private agencies. But when the publishing funds of all of these agencies became insufficient, near the end of the Nineteenth Century, all of them began to compete with one another for the securing of additional funds with which to finance the publishing of the increasing output of research. They did not inquire too carefully, apparently, into the adequacy of their method of solving the problem. Scholars, for instance, assumed that it would be possible to continue to receive royalties on the salable part of their literary product and to expect other agencies to raise the necessary funds for the rest of their product which commanded a limited and therefore unprofitable economic market. Scholarly societies seemed oblivious to the fact that it might be impossible to continue to offset rising printing and distributing costs and decreasing sales, because of specialization of markets and purchasing power, by economies or raising prices or additional subsidies.

Mr. Malone has just proposed that scholars should change many of their writing habits. He is scholar as well

as publisher and therefore qualified to speak so boldly. I found myself resorting to riddles. Why should scholars not also change their attitude toward the distribution of their literary product if they really wish to participate in an adequate solution of this problem which seems to concern them so deeply?

Scholars may well demur at this point in my argument to indicate that my position presupposes the assumption that there is an adequate solution to scholarly publishing problems. The lawyer's device of throwing the burden of proof onto the other party is a two-edged sword, and scholars may well ask, in other words: What is the nature of the solution of scholarly publishing which you propose and of the participation which is required of scholars in order to bring it about?

I shall be delighted if my riddle works that well, for those of you who know my position know that I am on record that scholarly publishing problems are not so complex or so difficult as to be incapable of solution. There is not only one way, there are several ways of solving them, and all I need to do now is to explain those methods concretely and my job is done. Intelligent groups like this audience, composed of scholars and publishers, would then volunteer their services to decide which is the better plan and which is the one which should receive their combined allegiance.

Here then are three perfect solutions of the publishing riddle:

1. *Full institutional responsibility.* The institution that provides the funds for research has most at stake. That bill is in itself nine-tenths of the total cost of preparing and distributing the results. Moreover, the institution furnishes most of the remaining tenth of the cost as it is the largest purchaser of scholarly books and journals for library shelves. Its research program suffers when manuscripts accumulate on the desks of the staff. In the period

following the close of the war, when the number of scholarly manuscripts increased so greatly that the load was too much for existing facilities, it was the educational institutions that raised most of the funds necessary to underwrite the publishing of the accumulated material, and that have continued to bear an increasingly greater share of the total cost of publishing the output of their staffs. My prediction in 1927 that institutions would be forced to assume a larger financial responsibility, and possibly a wholly new attitude toward the entire publishing problem, was not superior insight, but largely a knowledge of the mysteries of institutional budgets.

One research organization, The Brookings Institution, has led the way. You are well acquainted with its important and significant program. The Brookings Institution guarantees publication of the entire literary output of its staff. On the other hand, it is understood at the time of appointment that the entire output will be the property of the Institution. The organization maintains its own publishing activities at little or no net cost. So far as financial results are concerned, the program might be carried on equally successfully through outside publishing channels. The theory of the program is very simple. The principle might not work out in every institution, and it is entirely impractical until the compensation of scholars is adequate to obviate the necessity for supplementing their income from other sources. The theory underlying the program is, however, thoroughly defensible. There would be no scholarly publishing problem if all of the literary product of scholars of the country were included in one financial reckoning and if the understanding were general among scholars that they waived all royalties or other returns in return for a blanket publishing agreement for all of their product.

2. *Limited institutional responsibility.* There is another practical approach to the solution of the problem that does not require any fundamental change in the traditional

theory concerning the ownership of the literary product of the scholar. Assuming that the scholars of an institution continue to publish their texts and commercially profitable books through commercial channels and for their own benefit, is there any way by which the institution can solve the problem of underwriting the publication of the kind of manuscripts that most of these scholars now bring to university presses? Here again there is a wholly practical solution. A reserve of ten per cent of the money in the budget of the department, set up meticulously, in every department of an institution would enable the institution to assure its staff of adequate publication facilities for the part of the product that by definition was too limited in demand to find publication elsewhere. And here again the plan would work equally well either through publishing facilities at the institution or through subsidies to outside publishing facilities. This is another simple, practical solution, provided scholars would agree to waive that last ten per cent of the budget available for each department and any royalties or other profit sharing revenue from manuscripts which by definition are unprofitable.

3. *Strengthening existing publishing agencies.* Dr. Abraham Flexner suggests adequate endowments for university presses as still another solution. If the funds were truly unlimited the proposal might work, but the endowment would need to be larger than the Federal public debt if scholars at all institutions can increase their productivity as quickly as my colleagues can when they hear of unexpended balances of far smaller proportions. My concern for readers, librarians, and professional bibliographers, as well as these financial implications, have always prevented me from pressing this particular solution as the way out.

But there are other feasible ways of strengthening existing publishing agencies which will probably prove to be the most feasible way of solving our problem. Specifically, I think of several concrete suggestions.

Free publishing funds. Each of the larger publishing

agencies issuing scholarly material could probably spend ten, twenty-five, one hundred thousand dollars wisely in new publishing ventures which would actually revolve to the credit of the regular publishing activities of that organization. My institution made splendid use of such a grant of $100,000 from the Laura Spelman Rockefeller Memorial. The grant was made in 1919, just after the war, at a time when the situation was quite different from that of the present day. The conclusion of the war was a great stimulus to the completion of research, and the Editorial Board of my institution made a canvass of the entire University asking the head of each department to specify the most significant research which was unpublished, or was not being completed for want of assurances regarding publication. From these returns the Board selected specific titles and gave the necessary assurance regarding the publication of the finished results. The long list of publications issued in this way represents our chief claim for distinction during the dark decade. We are still in 1938 feeling the beneficial effects from that program.

Increased efficiency and improvements in distribution methods are other concrete means of strengthening and improving the contributions of existing agencies. There has been much talk today about reducing expenses and improving technical processes of reproducing scholarly material. These are important considerations for periodicals and very specialized books, but relatively unimportant for other types of publications. Economy may easily be carried too far in printing as in so many other fields. The newer auxiliary reproducing methods are extremely important for certain situations, but if universally used might increase the purchaser's outlay and the total cost, and also materially decrease the availability of valuable material. Improvements in distribution methods, by which I mean either reduction in the costs of placing scholarly material in the hands of the ultimate consumer, or improvement in

the effectiveness of promotion which results in reaching a larger market, particularly if it be a market outside the regular sale to institutional libraries, represent far greater potential contributions to the solution of our problem. Let no one assert in your presence that university presses, the scholarly publishing agencies that I know most about, are not efficient publishing organizations. You know something of their importance judged by standards of the scholarly world in which dividends are not the final measure of achievement, but my statement refers specifically to their financial efficiency judged by usual commercial standards. The showing of the leading university presses indicates that for dollar of capital invested, net cost per book, and percentage marketing returns, their methods will bear comparison with usual commercial standards. Two of the reasons for great improvement along these lines during the past ten years have been (1) "Dame Necessity" herself, and (2) a co-operative attack on their problems by an informal association of university presses that now numbers twenty. This group has remained in close association with the National Association of Book Publishers and has maintained a continuous exchange of information and study of the marketing methods of its members. A centralized mailing list, called the Educational Directory, serves all of these presses at greatly reduced costs as compared with the previous situation where each press maintained its own mailing lists.

A new commercial agency, University Books Incorporated, inaugurated by Farrar and Rinehart, seems to promise further economies in joint warehousing and the distribution of books to bookstores and possibly libraries.* The facilities would be difficult to maintain under co-operative financing and control, but the scholarly publishing agencies using those facilities are finding the solution to some of their problems by a more adequate distribution of their books through the general trade. The fact that a commer-

* The organization was unfortunately discontinued October 1, 1938.

cial publisher would finance such a venture is an example of the increasing appreciation by commercial publishers of the fact that strong university presses are an asset to the publishing industry. They no longer begrudge university presses any well earned measure of success so long as it is in proper proportion to their scholarly activities and the proceeds are used for the benefit of this function.

New types of scholarly books. Mr. Malone has argued for a change in the nature of scholarship in this country. Increasing numbers of scholars regard such a development as almost essential if scholarship itself is to prosper. If I have made my previous points clear, the publishing implications of that new type of scholarly product are also important. Interpretations of research written for large groups of scholars and general readers should be increasingly profitable from the economic point of view. A slight amount of imagination on the part of scholars would point to the desirability of directing the publishing of this new type of material into channels which would, if possible, contribute to the solution of some of the problems connected with the publication of the more difficult and more limited types of scholarly publishing.

Let me forestall at once any semblance of pressure or paternalism implied in this statement. By definition, I approach this part of my paper from the point of view of the extent to which scholars were really concerned with the solution of the publishing riddle, and my effort now, therefore, is merely to examine all possible approaches to the solution of the problem which might be made in that spirit. As a further precaution against a wrong interpretation of my position in this matter, let me briefly outline an experiment at my own institution that seems to indicate the feasibility of this suggestion and the beginning of a new attitude toward their literary efforts on the part of some of my colleagues. It is still too early to tell the final outcome of this new experiment but I consider its underlying philosophy

worth recording at this time. Several years ago, in addition to strengthening our own publishing program, we announced officially that the Editorial Board of the Press would, on request, consider manuscripts, not only from the point of view of quality of content, but also from the point of view of the most desirable publishing arrangements that could be made for that specific manuscript. The Press carefully redefined its publishing program to include certain definite publishing objectives, and decided to try to arrange for other types through joint publishing arrangements with commercial publishing houses. The plan was announced in the July 14, 1934, issue of *Publishers' Weekly* and has received cordial support from commercial publishers. Manuscripts handled under such arrangments benefit from the combined advantages of the University's editorial imprint and the editorial and marketing facilities of the commercial publisher selected for the particular project. The publishing contract in each case is approved by the author and provides for the usual author's royalties. The University's co-operation in the editorial and promotion aspects of these ventures is rewarded by the commercial publisher in the form of an editorial royalty or a share in the profits of these enterprises. The plan is still too new to draw any hard and fast conclusions regarding eventual success, but approximately one hundred and forty-three volumes have been published under such arrangements with ten commercial publishing houses. Some of the arrangements have more than justified the hope of better returns to the authors and to the publishers. The financial return to the University's scholarly publishing funds has not yet been in proportion to the results that might have been accomplished had the manuscripts been published by the Press itself, but the return has been substantial and is increasing yearly with the extension of the service. The economic philosophy of the plan, in other words, is sound from the point of view of this paper, and if

the plan fails it will be because of the subtle, intangible factors which wreck so many theoretically perfect programs.

Twenty years of association with scholars have taught me that they may be relied upon to further any sound cause in the interest of scholarship. I am not certain that the requisite co-operation and division of labor among the agencies publishing the scholarly work is possible. But that is another riddle.

ON THE POSSIBILITY OF A BIOLOGICAL MECHANISM CONTROLLING THE OCCURRENCE OF THE OXYGEN MINIMUM LAYER IN THE SEA

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(Communicated by Edwin G. Conklin)

ABSTRACT

A consideration of the densities of plankton particles determined by Seiwell and Seiwell (1938) indicates that no reasonable or even possible thermal expansion coefficient for such particles would permit them to find a level of equivalent density in the surrounding medium of a natural ocean, at which such particles would accumulate and create an increased oxygen consumption for their decomposition. A consideration of other factors involved in the settling and decomposition of plankton organisms also points quite definitely in the same direction on the basis of the density determinations of Seiwell and Seiwell.

If other organic particles should occur with a density differing in so slight a degree from the density of the medium that equivalent densities might be reached at deeper levels, this condition, with its attendant accumulation, must be attained at or above the level of greatest potential density of the surrounding seawater, that is, at or above the salinity maximum level. Since this is situated above the oxygen minimum layer, the possibility of explaining the latter by an accumulation of decomposing plankton particles seems excluded.

In a recent article in this series Seiwell and Seiwell (1938) have introduced some very interesting observations and speculations upon the marine-biologically extremely important factor of the sinking speed of decomposing plankton. While the authors carefully and explicitly refrain from suggesting that their experimental and deductive results give a true picture of actual events in nature, it is nevertheless tentatively generalized that "particles sinking through water of variable density will approach some level asymptotically" and that in consequence "a stratum of maximum oxygen consumption may be conceived of as occurring in the sea" through an accumulation of decomposing organic particles at the levels of asymptotic approach.

This tentative generalization further implies the possibility of a purely biological mechanism for the explanation of the occurrence of the oxygen minimum layer, which would be opposed to the theory held by Dietrich (1936; 1937a; 1937b), Wüst (1936), and many others, and to which the present writer also is inclined, namely, that the oxygen distribution, particularly as it refers to the oxygen minimum layer, is controlled largely by the horizontal and vertical circulation of the watermasses themselves. It therefore seems worth while to investigate the possibility of such a biological explanation further by a study of the premises from which it has been derived.

In their deductions Seiwell and Seiwell make use of Stoke's formula:

$$v = \frac{2g}{9\mu} r^2(\rho - \rho'),$$

in which v represents the rate of settling of a particle of radius, r, and density, ρ, through a medium of density, ρ', and viscosity, μ, under the influence of gravity, g.

In deducing the possibility of an asymptotic approach of the settling particle to a certain level determined by its size and rate of decomposition, Seiwell and Seiwell, while acknowledging that it is not strictly correct, make the assumption that the density of the particle, ρ, remains constant and only the density of the medium, ρ', changes with depth, so that a sufficiently large particle will settle asymptotically to a level of equivalent density where the term $(\rho - \rho')$ becomes 0.

Actually this assumption of a constant ρ would seem the entire basis for the deduction of an asymptotic approach to 0 settling velocity at any level above the one at which the radius of the particle, r, itself becomes 0 and the particle in other words no longer exists. A closer examination of the assumption of constant ρ is therefore required.

To the extent that the increase in the density of the seawater with depth is controlled by a reduction of temperatures, it is clear that the particle must be subject to similar increase

in density by thermal contraction. Since the particles with which we are here concerned are so small and their settling velocity so slight, it is perfectly safe to assume that there will be no significant lag effect between the temperature of the medium and the temperature of the particle. That is, the temperature of medium and particle will be identical at all levels. If κ is the mean cubical expansion coefficient of the particle over the temperature range considered and κ' the expansion coefficient of the medium, the expression:

$$(\rho_0 - \rho_0'),$$

applying to densities at the surface with temperature t_0, will at a depth d with temperature $t_d = t_0 + \Delta t$ [1] have changed to:

$$\left(\frac{\rho_0}{1 + \kappa\Delta t} - \frac{\rho_0'}{1 + \kappa'\Delta t}\right). \tag{1}$$

If we designate the density difference at the surface $(\rho_0 - \rho_0')$ as $\Delta\sigma_0$ and the density difference at depth d as $\Delta\sigma_d$, and substitute

$$\rho_0' = \rho_0 - \Delta\sigma_0 \qquad \text{in (1)},$$

we obtain

$$\Delta\sigma_d = \frac{\rho_0}{1 + \kappa\Delta t} - \frac{\rho_0 - \Delta\sigma_0}{1 + \kappa'\Delta t}$$

or

$$\Delta\sigma_d = \frac{\Delta\sigma_0(1 + \kappa\Delta t) - \rho_0\Delta t(\kappa - \kappa')}{(1 + \kappa\Delta t)(1 + \kappa'\Delta t)}. \tag{2}$$

The question is now whether with reasonable permissible values of κ and κ', and with the observed values for ρ_0 and $\Delta\sigma_0$ the value of $\Delta\sigma_d$ as expressed in equation (2) can ever approach 0 so that particles showing a $\Delta\sigma_0$ relationship to the density of the medium at a higher level could ever find a deeper level of equivalent density in the environment. Since the value of the denominator in (2) always remains positive (under all natural circumstances), and other than 0, we are

[1] Δt being negative in this case.

here only concerned with the enumerator

$$\Delta\sigma_0(1 + \kappa\Delta t) - \rho_0\Delta t(\kappa - \kappa'). \tag{2a}$$

On pages 469–472 Seiwell and Seiwell records experiments at temperatures of 22–25° C. with particles of a mean $\rho_0 = 1.02351$ and a $\Delta\sigma_0 = 0.00242$. In a natural settling through the ocean the temperature would decline by about 20° C. before bottom was reached (at 2–5° C.), giving $\Delta t = -20$. Inserting in (2a) and setting the expression equal to 0 we get

$$0.00242[1 + \kappa(-20)] - 1.02351(-20)(\kappa - \kappa') = 0$$

or

$$20.4218\kappa = 20.4702\kappa' - 0.00242,$$

and

$$\kappa = 1.00237\kappa' - 0.0001185. \tag{3}$$

The factor for κ' in (3) is obviously so near unity that it has no bearing upon whether it is reasonable to assume that a particle with 0.00242 specific gravity in *seawater* ($\Delta\sigma_0$) near the surface could find a level of equivalent density of the environment ($\Delta\sigma_d = 0$) if temperature alone controlled the density of both particle and specimen. Our interest therefore lies with the numerical term 0.0001185 (or 1.185×10^{-4})[1] which indicates that if particles of such density in seawater at higher levels as indicated in the experiments of Seiwell and Seiwell were to find a level of equivalent density deeper down their cubical thermal expansion coefficient would have to differ in this amount in a *negative* direction from the thermal expansion coefficient of the medium.

Since this value for the required difference between coefficients (1.185×10^{-4}) is on the *same order of magnitude as the coefficients themselves* for such substances and solutions as those with which we are here dealing, being about two-thirds of the mean cubical expansion coefficient of seawater (sal. 3.5 per cent) between 2° and 22° C., since furthermore organic particles of plankton origin are more than 90 per cent water

[1] It will be noted that this figure could actually be determined with sufficient accuracy for our purposes by simply dividing $\Delta\sigma_0$ by the temperature range through which the particle would pass before reaching bottom.

and the water-soluble body liquids in marine forms are approximately isotonic with the environment, it is obvious that the assumption that such particles might find a level of equivalent density where $\Delta\sigma_d$ becomes 0 requires the impossible collateral assumption that substances constituting only a small percentage of each particle could have (unknown) thermal expansion coefficients so different from that of the environment as to cause the *mean* expansion coefficient of the *entire* particle to differ from the environmental coefficient by an amount on the same order of magnitude as the latter itself.

Therefore it is reasonable to conclude that if density differences in the third decimal place are observed between organic particles and environment at any level, such particles will not be able to find equivalent densities in the medium at any deeper level so far as temperature-controlled variations in density under natural conditions in the ocean are concerned. This probably also applies to differences in fourth decimal place, at least to the higher values in this bracket. The cases considered by Seiwell and Seiwell both deal with differences in the third decimal place, the assumed $\Delta\sigma_0$ in the case of Atlantis station 1170 being more than 0.0035.

The only remaining possibility is therefore that chemically-controlled variations in the density of the medium might introduce asymptote levels at which the settling particles might find equivalent densities and be brought to a stop. But if this should be the case, it is obvious that the layer of accumulation should coincide with the *maximum salinity* layer, and that it is extremely improbable that any particle which has penetrated this layer could be brought to a stop farther down, since the potential density at equal temperatures of the medium at lower levels is less than that at the salinity maximum. Since the *oxygen minimum* layer in the Atlantic is well below the salinity maximum the possibility therefore seems excluded that the oxygen minimum could develop as a result of an accumulation of decomposing organic particles which have managed to pass through the salinity maximum layer above.

In connection with chemically-controlled variations in density one must again point to the fact that marine plankton organisms even in life tend to be isotonic with the medium. After death and beginning decomposition they should, of course, become still more open to osmotic effects, and if they have more persistent shells than soft parts one must even expect a purely mechanical invasion of their husks as the more readily decomposed substances go into solution. There is therefore good reason to assume that even the chemically-controlled component in the density of the settling particle will to a greater or less extent vary in a similar manner as the chemically-controlled density variations of the environment.

With full recognition of the fact that variations in sinking speed must occur to some extent with variations in density, viscosity, size of particles, etc., it therefore would seem more reasonable in dealing with the problem in first approximation on an essentially speculative basis only, to assume that the entire expression $(\rho_0 - \rho_0')$ remains constant rather than that ρ_0 remains constant while ρ_0' varies. It definitely seems very improbable that particles showing density differences from the medium in the third, or even in the higher values of the fourth, decimal place can approach asymptotically any level higher than their depth of complete disintegration. And, in conclusion, it seems virtually excluded that particles with such specific gravities in seawater at higher levels as those recorded and considered by Seiwell and Seiwell could possibly accumulate where the oxygen minimum occurs, if they were able to pass the salinity maximum.

In conclusion it might be mentioned that if the settling velocity is a function of the second power of the linear dimensions of the particle, $f(r^2)$, the time spent by the particle in each depth integral is the inverse of this function, or $\frac{1}{f(r^2)}$; and, if according to a general principle, we assume with Seiwell and Seiwell that the rate of oxidation is directly proportional to the surface of reaction, $f(r^2)$, then the oxygen consumed by each particle per unit depth traversed remains constant and

unaffected by its diminishing size, under otherwise equal circumstances. This further means that if we assume $(\rho_0 - \rho_0')$ to remain approximately constant and take the changes in viscosity of seawater (3.5 per cent) quoted by Murray and Hjort (1912, p. 690), from Ostwald, between 25° and 5°, then the velocity of the particle when it reaches 5° temperature will stand in a ratio to its velocity at 25° of

$$\frac{\frac{r_5^2}{88}}{\frac{r_{25}^2}{53}} = \frac{53r_5^2}{88r_{25}^2} = R_{s_{5/25}}.$$

And if the rate of oxidation according to the general rough average for chemical and biochemical processes is approximately doubled by a 10° increase in temperature and is also directly proportional to the surface of reaction, the rate of oxidation per unit *time* at 5° would stand in a ratio to the rate at 25° of

$$\frac{r_5^2 \times 1/4}{r_{25}^2 \times 1} = \frac{r_5^2}{4r_{25}^2} = R_{0_{5/25}}$$

and the ratio of the rates of oxygen consumption per unit depth (R_0') would be:

$$\frac{R_{0_5}'}{R_{0_{25}}'} = R_{0_{5/25}} \times \frac{1}{R_{s_{5/25}}} = \frac{r_5^2}{4r_{25}^2} \times \frac{88r_{25}^2}{53r_5^2} = \frac{88}{4 \times 53} = 0.4.$$

That is, on these approximate simplifying assumptions, there should be a constant reduction in oxygen consumption per unit depth by each particle settling through a natural ocean. Although the over-simplification of the premises (*e.g.*, by leaving pressure effects upon viscosity etc. out of consideration) makes the numerical result too inaccurate to be of quantitative significance, it may nevertheless be taken to point qualitatively in the right direction, that is towards a diminishing oxygen consumption per unit depth caused by the settling particles.

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ENGLISH ANTECEDENTS OF VIRGINIA ARCHITECTURE

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(Communicated by Leicester B. Holland)

(*Read April 23, 1938*)

Abstract

In the study of the early architecture of the American Colonies the English antecedents of informal building have been generally recognised. The possible close connection, however, between academic architecture in the colonies and in England has not been fully realised or the actual connections established. In this study a certain group of buildings, built in Virginia in the first quarter of the 18th century, under the aegis of Robert Carter of Corotoman, has been examined and the probable English antecedents explored. The locale of the probable prototypes of the American buildings is Shropshire. The architect working in this area who may have been the author of both groups was John Prince of Shrewsbury. The lack of documents in Virginia archives relative to Carter's buildings, due to losses by fire and wartime depredations has prevented documentary research into the authorship of the designs. The architectural evidence relating the buildings to each other both in Virginia and Shropshire is impressive. Possibly as yet undiscovered early papers in Virginia and Shropshire collections may document the conclusions reached here.

In the building of the first hundred years in the colony of Virginia, it is obvious that English tradition must have been paramount. Unless influenced by different climatic conditions, or by new building materials, emigrants will build in the architectural tradition of their mother country. However, by the end of a century's separation the building might be expected to take on a new character, and this the minor architecture of Virginia actually did.

The 17th century was a time of change in England as well as in Virginia. At its beginning the great mansions and public buildings were actually late mediæval in style, but by 1700 the influence of the Renaissance was ascendant and the corresponding structures had become almost Georgian. The builders of Williamsburg, the capital of Virginia, as well as the

builders of the great county houses of the period, were well aware that the every day architectural style of the colony was old fashioned. Naturally they looked to England for new ideas in building and English designs came to them through publications, through architects, or through builders.

Virginia was and is agrarian, and it is in the great country houses and the rural churches and court houses, that its characteristic building expression is found. Of the two latter groups many examples remain. In the case of the churches the records of the vestries frequently survive to assist the study of the buildings. When the county records escaped burning in the wars they form a valuable record for the study of early court houses. Both of these fields are limited; it is known definitely how many structures of each were built and where they were located. Of the mansions and lesser domestic buildings the field is unlimited; a vast number were built and while many exist (though almost none without alteration) the larger part are gone sometimes without trace or tradition of their existence or location.

In the field of the major domestic architecture of early Virginia it is possible to attempt a classification based on the relationship to English architectural types.

For the present purpose we shall discuss only one Virginia group and its probable English antecedents. This can be called Shropshire School because of its unmistakable use of the architectural idiom of Shropshire. This school seems to have had as its sponsor in Virginia one of the most interesting of Colonial personages, Robert (called King) Carter.

Just how the Shropshire architectural influence came to be exerted upon Robert Carter is uncertain, but it is not unreasonable that it should have been so, as this county was one of the most important in the English background of Virginia families. In Swem's Index of Virginia History, fourteen prominent families are shown to have come from Shropshire and many from the neighboring counties of Hereford and Cheshire. Dr. E. J. Lee, the biographer of the Lees, traces the American branch of the family to Coton in Shrop-

shire, and in support of this origin many 18th century letters survive from the correspondence between the American and English branches of the family.

The possibility that other distinguished Virginia families, whose English antecedents are indeterminate, originally came from Shropshire is shown by two names (later borne by prominent Virginians) in a list of the mayors of Chester, England, of the 16th and 17th centuries. These names are on a panel in St. John's Church, Chester, and the armorial bearings which are given as well are similar to the Virginia families, with the usual differentiation showing the senior and cadet lines of the family. The first of these names is that of William Byrd, perhaps an ancestor of the Byrds of Westover, or at least indubitably a connection. The second is that of Matthew Page. A colonist of the same name was the father of Mann Page who married Judith, daughter of Robert Carter.

The Hills of Court Hill, Shropshire, have tentatively been identified as the ancestors of Elizabeth Hill of Shirly, wife of John, eldest son of Robert Carter. The English origin of the Carters themselves is obscure, but Capt. R. R. Carter, an authority on the subject, believes them to be of Carstown, Herefordshire, not far from Crednal, home of Anne Landon, Robert Carter's second wife. These multiple relationships between Shropshire and the adjacent counties, and Virginia, indicate that architectural similarities are to be expected.

Robert Carter, an indefatigable builder, was an excellent instrument for the dissemination of Shropshire influence in Virginia building. His own great mansion of Corotoman, was built during the first quarter of the 18th century, as in his will of 1726 Carter says, "I give unto my son John, all my furniture in the new house and half of the rest of the furniture in my kitchen and other houses about my mansion Dwelling." No further information regarding the date of building has come to light, but we know from the Maryland Gazette of February 4, 1729, that "The . . . fine large house of Col. Carter on Rappahannock was . . . lately burnt."

This is the same issue that recorded the burning of Mt. Pleasant, the predecessor of Stratford, the Lee mansion. It may be assumed that Carter began the building of Corotoman about 1702 as in addition to his large private estate, he was then in funds from his many public offices, as well as from the Fairfax estate, for which he was agent for vast Virginia properties, amounting to about five million acres. After the fire of 1729 the house does not seem to have been rebuilt, the family perhaps living in the modest frame building destroyed in 1929, which fitted the inventory of Robert Carter's estate in 1732. Beside Corotoman he is said to have built Sabine Hall for his son Landon, Nomini Hall for Robert, Cleve for Charles and perhaps Carter's Grove for his daughter Elizabeth, though none of these mansions were completed before his death.

One thing is certain, he did build, about 1725 or 1730, Christ Church, Lancaster County. This was his parish church and he reserved a quarter of it for his family and servants. The building is very stylized with much finely worked brick and stone trim. The architectural treatment is unlike that of any other church in Virginia both in general disposition and detail, and is the only one that has, without dispute, an actual English character. We might hope to find a prototype in England, or more particularly in Shropshire for Christ Church, but church building of this period is exceedingly rare except in cities, because of the vast heritage of Gothic churches that England had received from the past. The cruciform plan of Christ Church, the monumental doorways and windows, the full entablature at the roof line and the tall hipped roof all speak strongly of an English origin for the design.

The superlative quality of the brick and stonework alone would imply a relationship to another Virginia building which would give us a clue to the authorship of Christ Church. Moreover, the parallel in the design of the great arched windows is so extraordinary as to demonstrate that the two buildings are by the same architect. In all the details the marked similarities can be observed; the moulded stone sills,

the rubbed dressings in the brickwork, the impost and keys, the dimensions and the sash treatment. The parallel building is Rosewell, the mansion built by Mann Page for his wife, Judith Carter, between 1720 and 1730, and toward the furnishing of which Robert Carter devised a hundred pounds in his will of 1726.

Rosewell is the largest and most magnificent house known to have been built in the American Colonies. The building was so heavy a drain upon the resources of the estate, which at the time comprised a vast acreage, that the entail had to be broken by an act of the Virginia Assembly in 1744 to enable the heir of the builder to sell land to pay the debts incurred in its construction. The house remained in the family for over a hundred years but about 1835 was sold, the purchaser stripping the house of all saleable parts and modernizing the interior. In 1916 the building was burned to the ground, only the massive brick walls remaining. Fortunately we possess in addition to fine photographs of the stairway an excellent and spirited description of the mansion before the sale, by Anne Page Saunders, of Williamsburg. This was contained in her reminiscences of Christmas holidays spent at Rosewell as a young girl, which were published in Baltimore under the title "Leonora and the Ghost" in 1872.

The design of both exterior and interior of the house represented a provincial handling of Sir Christopher Wren's mannerisms. It was three full stories high in addition to having the first floor raised six feet above the ground. The doorways and windows had richly worked stone and brick trim, closely paralleling that of Christ Church. The interior was no less elaborate, the stair being of mahogany, intricately carved. In addition to the photographs several fragments from it which were removed after the sale remain at Shelly, a neighboring Page house. According to a record preserved in the *Southern Literary Messenger*, the entrance hall was hung with tapestries. Mrs. Saunders describes the waxed mahogany of the wainscot, the mantles of various colored marbles in the different rooms, and the damask hangings.

Rosewell was unique in the Colonies and it had an unique plan. The only other building in which this plan recurs, as far as it has been possible to determine, is Cound Hall in Shropshire. This was built for the Pelham family in 1703 by John Prince of Shrewsbury, who signed and dated the drawing of the mansion, still preserved there. The elements of the plan are identical; a large entrance stair hall in one corner of the building, and a large room at each of the other three corners. The two rooms away from the entrance front are separated by a hall at Rosewell, which at Cound, on account of the greater length of the house, becomes a salon. The location of the chimneys is inside the end walls, with closets in the space between them and the corners of the buildings. Similar closets on the inner side of the chimneys at Cound become small rooms through great projecting end pavillions.

On account of the marked similarities of the plans, the resemblances of the exterior designs become significant and make it seem possible that Prince was the designer of both houses. Unfortunately nothing is known of his works other than Cound, and little of his life except that he lived in Shrewsbury. An analysis of Cound leads one to believe that Prince was a man from the country whose architectural training was derived through published architectural works. This alone would account for solecisms in the design of Cound, such as the use of colossal stone pilasters.

In Mrs. Stackhouse Acton's book on Shropshire houses there is a drawing of Soulton Hall, about ten miles from Shrewsbury. The house was built about 1670 by the Hill family, a connection of the Hills of Court Hill, with whom Elizabeth Carter has been tentatively identified. The plan displays the same individualities as those of Cound and Rosewell, though the great end pavilions do not occur. In the exterior is found the same three story facade. Banks of casements take the place of the tall double-hung sash of the two later houses but as sash windows were first used in England, according to Sir Lawrence Weaver, in 1672 at Groomsbridge

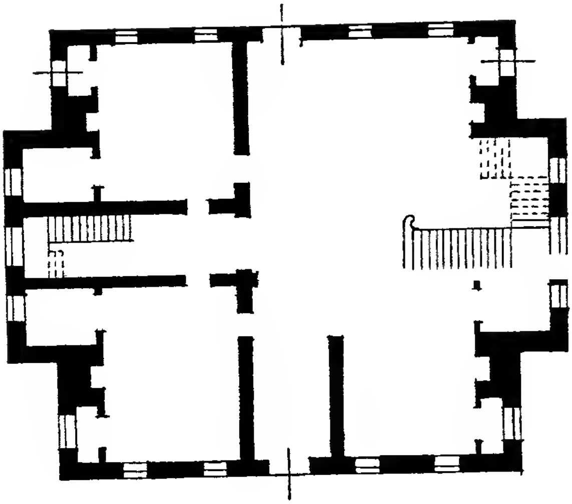
Place, their non-use at Soulton was occasioned by current practice rather than style.

A link between Soulton, and Cound and Rosewell, perhaps may be established through a painting of a late Stuart mansion preserved from a paneled overmantel formerly at Morattico Hall, Richmond County, Virginia. This latter house, not far from Corotoman, was built in the early 18th century by the Griffin family who were closely allied with the Carters through marriage and business. The house, or perhaps a dependency of the mansion, stood until about 1928 when it was destroyed by erosion of the bank of the Rappahannock River.

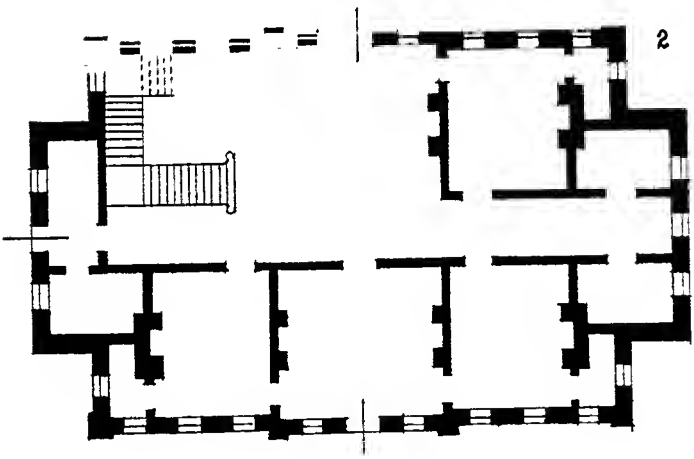
The painting, on a narrow panel eight feet long, though crudely delineated, is quite evidently a portrait, not an idealized picture, because of the extraordinary detail into which the artist has gone. In it one can immediately see the resemblance to Rosewell and Cound. The house is shown four stories high with the entrance through the ground floor and the first floor developed as the "piano nobile." Superimposed orders of pilasters ornament the two middle floors, in contrast to the colossal pilasters at Cound and the absence of all major ornament at Rosewell. It seems very possible that this may prove to be Corotoman or at least a work of John Prince, intermediate between Cound and Rosewell.

The strong architectural parallel between these two latter mansions, combined with the Shropshire affiliations of the Carter family, will have to substitute, for the present at least, for documentary evidence of their relationship. In spite of the fact that assiduous search has failed to disclose such evidence, this may ultimately come to light and establish the fact that Prince was actually the founder of the Shropshire school of Virginia building.

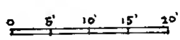
PLATE I



1



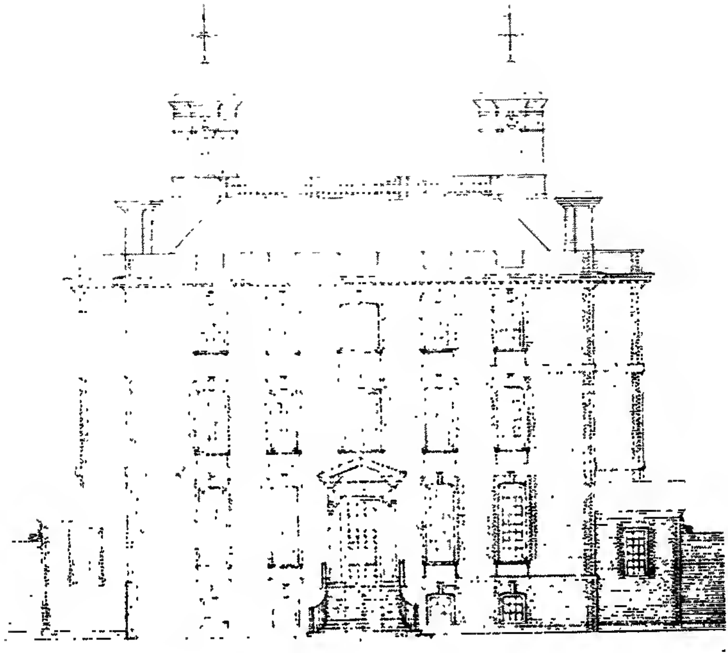
2



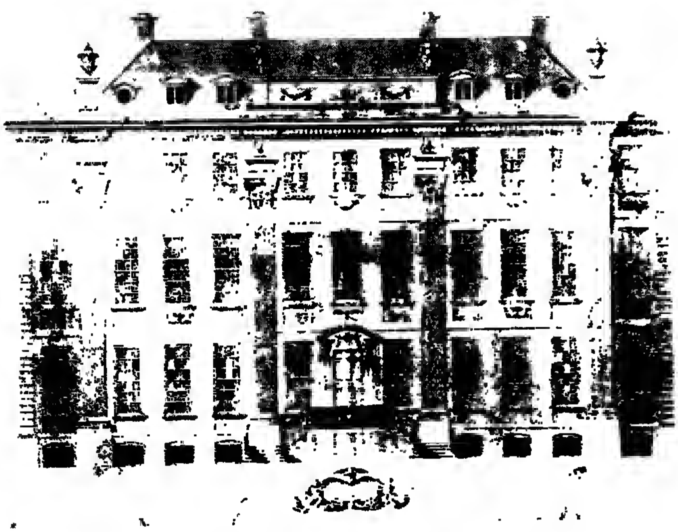
1. Rosewell (1720–30), Gloucester County, Virginia. *(Shown without dependencies.)*

2. Cound Hall (1703), Shropshire, England. *(Before the alterations.)*

PLATE II



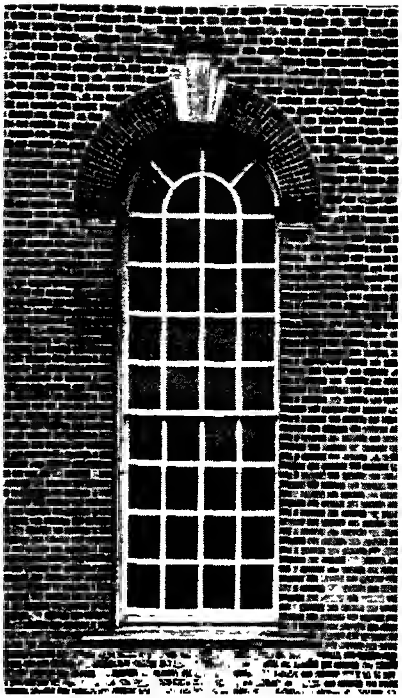
Rosewell, Gloucester County, Virginia.
(Restored Elevation) From Waterman & Barrows, "Domestic Colonial Architecture of Tidewater Virginia." Courtesy of Chas Scribner's Sons.



Cound Hall, Shropshire, England.
(From the Original Drawing of 1703) John Prince, Architect.

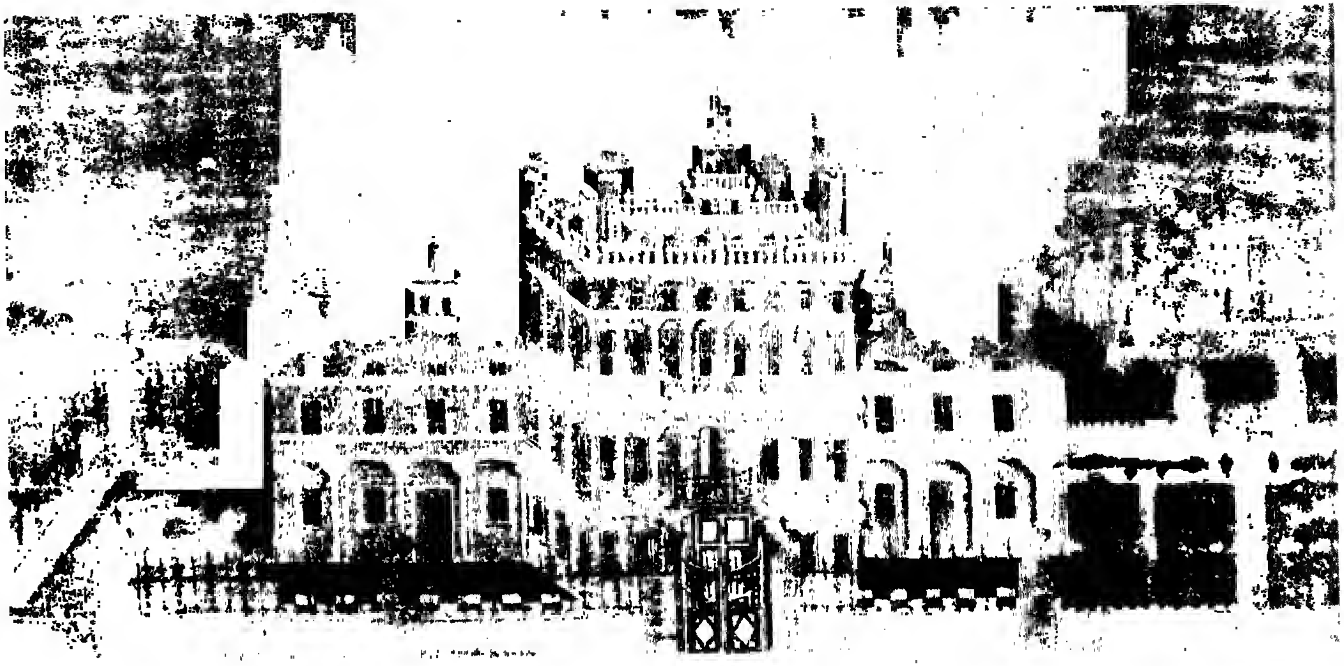
PLATE III

Rosewell (1720-30), Gloucester County, Virginia.
(Before the Fire.)



Christ Church (1732), Lancaster County, Virginia.

PLATE IV



Unidentified House from Overmantle Panel from Morattico Hall, Richmond County, Virginia.
(First Quarter of the Eighteenth Century) *Perhaps Corotoman, Lancaster County, Virginia.*

MUTANT BODY COLORS IN THE PARASITIC WASP *HABROBRACON JUGLANDIS* (ASHM.) AND THEIR BEHAVIOR IN MULTIPLE RECESSIVES AND IN MOSAICS

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(Communicated by J. Percy Moore)

ABSTRACT

Wild type individuals of the parasitic wasp *Habrobracon juglandis* vary from honey yellow to almost black. This is due primarily to temperature, higher producing more yellow, lower, more black. Heredity plays some part for races under constant temperature may differ consistently in pigmentation. Wild type males and those of the three body color mutant types, *black*, *honey* and *lemon*, as well as the four combinations of these, were reared under four conditions of temperature and humidity. These were *A*, 30° C., 33–39 per cent relative humidity; *B*, 30° C., 81 per cent; *C*, 19° C., 33 per cent and *D*, 19° C., 81 per cent. Wasps in *A* and *B* developed at the same rate and were alike in pigmentation and size, those in *C* and *D* were slower due to lower temperature. Those in *C* were slower than *D*, of the same color, but larger in size. Any lightening which the shorter cycle of *D* may have caused was cancelled by darkening due to smaller size.

It was found that more black pigment is produced at lower temperatures in all types except *honey*; that increase of black pigment due either to environmental or to genetic factors is accompanied by decrease in intensity of yellow. These facts are discussed in light of Wright's theory of pigment formation. He postulates two enzymes, I, necessary for all color, which, acting in combination with chromogen, produces yellow, and II, which can act only with I and, as I–II with chromogen, produces black. By supposing that *honey* represents a reduction of enzyme II, *black* an increase of II and *lemon* a reduction of I, the appearance of the multiple recessives can be predicted as well as their behavior under different temperature conditions.

Animals which are mosaic for body colors are described. In all combinations obtained thus far, each region is autonomous.

INTRODUCTION

SPECIMENS of *Habrobracon* taken in the field exhibit so wide a range of variation in color as to be very confusing to the

* The author is deeply indebted to the Department of Zoology, University of Pennsylvania, for space and facilities for carrying on this research, to the Department of Anatomy of the University Medical School for use of a cold constant temperature room, to the American Philosophical Society for a grant from the Penrose Fund and to Mr. Norris Jones of Swarthmore College whose patient and painstaking studies have resulted in the beautiful and accurate colored plates.

taxonomist. Muesebeck (1925) writes, "In a consideration of specific characters in *Microbracon* (*Habrobracon*) one is impressed by the lack of constancy in color or even color pattern, although sometimes there is a degree of uniformity which is of a little help and permits the employment of color characters to a small extent in a table to species." Whiting (1918) notes that, in *Habrobracon juglandis* (Ashm.), intensity of pigmentation does not yield to selection and suggests that it is probably due to some environmental influence. He also observes that smaller individuals are darker than larger. In 1921 he concludes that variation appears to be due almost wholly to temperature, high temperatures producing more yellow, low more black. At about the same time Hase (1922), in a very comprehensive and interesting paper, makes a similar observation. Genieys (1925) in an exhaustive study of *Habrobracon brevicornis* (Wesm.), a closely related species, notes that the action of heat on the coloration of adults is very remarkable. In addition, he suggests that increased humidity lightens color.

Schlottke (1926) from a carefully planned and controlled study of temperature and pigment interrelationships in wild type *Habrobracon juglandis* draws the following conclusions. (1) Deposition of pigment (black) decreases linearly with rising temperature: males are, on the average, darker than females. (2) Pigment is deposited especially at points of muscle attachment and the last parts to become light in higher temperatures are the regions where muscles attach vertically. (3) Animals bred at lower temperatures are larger and darker than those at higher but at a given temperature smallest animals are the darkest. (4) Changes in temperature at any time between egg stage four days before laying and prepupal stage affect adult coloration. In a later study (1934) he shows that increase or decrease of oxygen content of the atmosphere increases pigmentation and concludes that alteration of oxygen concentration influences the oxidations concerned with pigment formation indirectly as a non-specific stimulus on the organism as a whole.

Schlottke uses the pattern of the dorsal side of the meso-

thorax as an aid in classifying degrees of pigmentation since it is consistently related to muscle areas and is not complicated by other factors. Use of this method has been made in the present paper. Reference to Fig. 1 will make clear the main sclerites of the dorsal side of the mesothorax, to Fig. 2 the

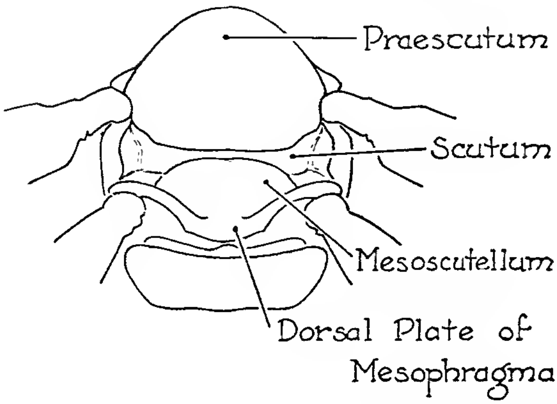

FIG. 1. Outline of dorsal sclerites of the mesothorax. (× 44)

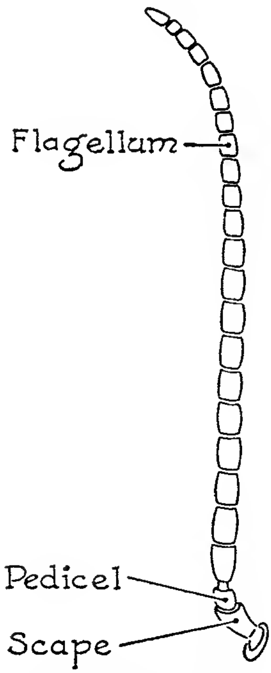

FIG. 2. Outline of male antenna. (× 44)

parts of an antenna, also used frequently in descriptions in the present paper.

That environment is not the sole element in color variation of wild type wasps has been amply demonstrated. Whiting (1925) described sooty mesosternum which, although temperature affected, appeared in one stock and not others under similar environmental conditions. Kühn (1927) showed by selection and crossing that heredity plays a considerable part in color determination, that different races under constant temperature conditions differ from each other consistently in intensity of pigmentation. He argues for an hereditary cytoplasmic influence. Whiting (1932) observed that, in spite of susceptibility of pigmentation to change, the great majority of individuals are symmetrical in color pattern and that in specimens mosaic for various mutant traits there appeared a high degree of asymmetry of wild type pigmentation indicating the existence of genetic factors for wild type pattern differences.

Technique

The technique of rearing the wasps discussed in this paper was as follows. Four large glass jars with tightly fitted covers were used, two (*A* and *B*) placed in an incubator regulated for about 30° C., the other two (*C* and *D*) in one in which the temperature was approximately 19° C. *A* was used with the top resting loosely on it so that the relative humidity would be that of the air in the incubator (33–39 per cent). Into *B* was put a saturated solution of $(NH_4)_2SO_4$ which at 30° keeps the enclosed atmosphere at 81 per cent relative humidity. *C* contained a saturated solution of $CaCl_2$ giving a relative humidity of 33 per cent at 19° while *D*, like *B*, contained $(NH_4)_2SO_4$, producing 81 per cent relative humidity at 19° also. Na_2SO_4 had been tried first for the cooler damper environment but 93 per cent saturation was found to be too wet and rapidly developing mold killed the wasps.

The same stock was used for each genetic type under the four conditions. Adult females were placed singly in shell

vials and each was given one caterpillar of the flour moth, *Ephestia kühniella* Zeller. The vials were covered by small squares of thin cheese cloth held in place by rubber bands. Those females which were to be put in the cold were given caterpillars already stung by other females, with due care to avoid contamination. This was done because wasps are so slowed up by the lower temperature that caterpillars are sometimes able to spin in or to entangle the females before being stung and paralyzed. The shell vials were kept in the respective jars and all eggs laid during the first four days were removed and discarded with the remainder of the first caterpillars. This was considered necessary because of Schlottke's findings on susceptibility of the unlaid eggs to temperature changes. Five large fresh caterpillars were then added and the vials left undisturbed until progeny emerged, except for the removal of the female after hosts were well covered with young maggots and eggs. Adult progeny were placed in 95 per cent ethyl alcohol for future study. Comparison of these with living individuals showed that the alcohol caused no change in color.

The specimens pictured and described in detail are males from jars *A* and *C*, 30° and 19° dry. The general effects of humidity are to be discussed in a later section of the paper. In describing the genetic types reared at high and low temperatures reference will be made to the colored plates for dorsal structures and for exact color, to Table 1 for a summary of the ventral aspects. Since it would be extremely difficult to distinguish in words differences in shades of yellow or brown, these adjectives are used without being qualified. The correct shade can be determined for each type by referring to its colored plate since colors are similar on dorsal and ventral sides of head and thorax. In all types the abdomen is covered, ventrally and laterally, by a cuticle which is thin and transparent except for small sclerotized regions at the lateral ends of the abdominal sternites. These are visibly distinct and often characteristically pigmented. The clypeus is a small inverted triangular sclerite just above the mandibles.

The space between this and the antennal region is known as the supraclypeal area.

Wild Type

Wild type wasps bred at 16° are almost entirely black. There is a pale yellow border around the eyes. The thorax shows restricted yellow in the praescutum only and may occasionally appear entirely black in smaller animals. The abdomen is black dorsally except for sooty anterior segments and shows black sternal thickenings on the ventral side. Legs are black with a slight paleness around some of the joints. Specimens bred at 35° show almost no black on the body but are instead a clear honey yellow, slightly darkened to brownish yellow in some areas. Antennæ, wings, feet, and, in the female, ovipositor sheath, remain black. Eye color behaves entirely independently. The wild type eyes are black at 30° but lighten to a dark reddish brown at 19°. Tips of mandibles are dark brown and remain unchanged at all temperatures. Between 16° and 35° the relative amounts of yellow and black vary but always with the points of muscle attachment and the stemmaticum (triangular patch between the simple eyes or ocelli) the last to fade and with some variation among individuals reared under the same conditions.

Pl. I A shows a wild type male reared at 30°. At this temperature ten days are required to complete a generation. A median black area is present on the praescutum near the anterior edge and lateral ones near its posterior margin. Ventrally, as recorded in Table 1, the head and thorax are clear yellow, the latter with some black, and the abdomen colorless with sternal thickenings faintly suggested and genitalia a pale gray. Pl. I B was made from a wild type male reared at 19° under which conditions twenty-seven days are necessary for a generation. Some smaller individuals in this group showed the thorax entirely black. Dissection of these specimens discloses that the characteristic yellow pattern of praescutum and mesoscutellum at higher temperatures is present in the cuticle as a "ghost pattern." Ventrally, the head is yellow as

TABLE 1

COLORS OF VENTRAL PARTS OF THE DIFFERENT GENETIC TYPES AT 30° AND 19° C.

	Head					Mesosternum		Abdomen	
	Mandibles	Teeth	Maxillæ	Clypeus	Supra-clypeal Area	Background	Pattern	Sternal Thickenings	Genitalia
+ 30°.	Yellow	Brown	Yellow	Yellow	Yellow	Yellow	Black	Cream	Gray
19°..	Yellow	Brown	Yellow	Yellow	Yellow	Yellow	Black	Black	Black
Bl. 30°..	Yellow	Black	Black	Black	Black	Yellow	Black	Gray	Black
19°..	Black	Black	Black	Black	Black	Black	Black	Black	Black
Ho. 30°..	Yellow	Brown	Yellow	Yellow	Yellow	Yellow	Brown	Cream	Brown
19°..	Yellow	Brown	Yellow	Brown	Yellow	Brown	Brown	Brown	Brown
Le. 30°.	Yellow	Brown	Yellow	Yellow	Yellow	Yellow	Yellow	Colorless	Yellow
19°..	Yellow	Brown	Yellow	Yellow	Black	Yellow	Black	Yellow	Gray
Ho. bl. 30°.	Yellow	Black	Brown	Yellow	Yellow	Yellow	Brown	Cream	Brown
19°..	Cream	Black	Brown	Brown	Brown	Yellow	Brown	Brown	Brown
Le. bl. 30°.	Yellow	Black	Black	Yellow	Yellow	Yellow	Gray	Cream	Gray
19°	Yellow	Black	Black	Black	Yellow	Yellow	Black	Cream	Black
Le. ho. 30°.	Yellow	Brown	Yellow	Yellow	Yellow	Yellow	Yellow	Colorless	Yellow
19°.	Yellow	Brown	Yellow	Yellow	Yellow	Yellow	Yellow	Cream	Yellow
Le. ho. bl. 30°..	Yellow	Black	Gray	Yellow	Yellow	Yellow	Yellow	Colorless	Yellow
19°.	Yellow	Black	Brown	Yellow	Yellow	Yellow	Brown	Brown	Brown

are the mandibles and maxillæ. The thorax is almost entirely black and yellow sternal thickenings outlined with black at their anterior inner edges may be seen on the abdomen. External genitalia are black.

It is to be noted that, as black becomes more widespread and intense, yellow becomes paler, not strikingly so in wild type, but to a degree easily observable.

Theoretical Aspects

With these facts concerning wild type in mind and as a preparation for discussion of mutant body colors, attention is called to some of the more or less theoretical aspects of pigment formation. The pigments of the stemmaticum of *Habrobracon* are granular and present in hypodermal cells beneath clear cuticle, as pointed out by Genieys for *Habrobracon brevicornis*. The other pigments, ranging from pale yellow through brown to black, are in the cuticle and appear to be non-granular. Both granular and diffuse pigments are similarly affected by temperature changes and undoubtedly belong to the melanins. Much remains to be determined about these compounds but certain facts have been established through experimental work. The melanins are formed from colorless substances or chromogens with the aid of enzymes in the presence of oxygen.

As long ago as 1917 Wright formulated a theory of color formation by combining these known chemical facts with a careful genetic analysis of pigments in mammalian hairs. Although insect cuticle may seem to be far removed from mammalian hairs, it has been found that Wright's theory fits the facts as observed in *Habrobracon*, fits them so well that, using it as a basis, the appearance of the double and triple recessives was predicted with a considerable degree of accuracy before they were actually obtained. According to Wright's theory the colorless chromogen is formed in cytoplasm and two enzymes or their forerunners in the nucleus. (For the sake of convenience the term enzyme will be used in this paper.) The union of these substances in the cytoplasm in differing amounts and under different conditions gives rise to different colors and intensities of color.

Chromogen and enzyme I (and O_2) are necessary for any color production; acting together they produce yellow. Enzyme II has no effect alone but in combination with I oxidizes

chromogen to black and is effective below the threshold of I alone. Red is associated with prolonged action or a greater amount of I, brown with modified behavior or a reduced amount of II. In the absence of I no color can be produced, in the absence of II, no black. An increase of I may intensify yellow or black or both, or increase black at the expense of yellow, depending on the amount of II available. Decrease in I will lighten both yellow and black, yellow directly, black through less I to combine with II. Increase in II will increase black at the expense of yellow since it will combine with more I while its decrease will result in less black and more yellow.

It will likewise follow that under a given set of conditions in the cell any environmental factor which increases the amount of black will result in a corresponding loss of yellow as more enzyme I will have been combined with II leaving less to act directly in producing yellow. In wild type, as noted above, lower temperatures intensify and extend black and with this goes a lightening of yellow.

Mutant Types

In the course of genetic studies on *Habrobracon juglandis* three body color mutations, *black* (bl.), *honey* (ho.) and *lemon* (le.), have been discovered. (Names of mutant stocks will be italicized to distinguish them from the colors as such.) These are all fully fertile and can be kept as homozygous stocks, singly or in any desired combination. Linkage tests have demonstrated that, by a coincidence, the genes for these body colors are in the same group. Crossover percentages are fourteen between *black* and *lemon*, six between *lemon* and *cantaloup* eye color, and nineteen between *cantaloup* and *honey*. (Unpublished data.)

Black

Dr. W. F. Dunning found among descendents of x-rayed material, the recessive mutant *black* (bl.). Later Miss Gail Stocker obtained a recurrence of the same gene among progeny of an x-rayed mated female. The factor blackens the animal

to an extreme degree, even at higher temperatures. Pl. II C represents a *black* male reared at 30°. The pattern is similar to that of wild type at 21°–22° but yellow areas are considerably lighter, almost cream in fact. It will be noted that the stemmaticum remains very black while the praescutum differs from that of wild type at a lower temperature in the shadowy continuation of the median patch to the posterior edge. Dissection shows this pattern to be entirely within the cuticle. Legs are completely black and wings and antennae darkened. The whole animal presents a glistening jet-like appearance which becomes even more striking at 19° (Pl. II D). Dissection reveals in the praescutum a ghost pattern corresponding to the light areas of higher temperatures but no trace of the light spots characteristic of the mesoscutellum. Mandibular teeth are black.

No difficulties are encountered in separating *black* from wild type whatever the conditions of rearing, for the final check is always extent and intensity of yellow regions on head and legs. They remain more extensive and a deeper yellow in wild type.

We may suppose that there has occurred in *black* an increase in enzyme II over that of wild type. This extends and intensifies black and in so doing reduces yellow to a striking degree. At lower temperatures in fact no light areas can be described as truly yellow. All have become a sooty or smoky cream.

Honey

In 1932 Dr. Kathryn Speicher obtained from a type female eight daughters and six sons. One of these males lacked black pigment entirely. This was found to be hereditary and recessive and the trait was called *honey* (ho.). Reference to Pls. III E and III F will show that black pigment is everywhere absent, even in animals reared at the lower temperature. A praescutal pattern similar to wild type may be observed but this is represented in darker yellow or red instead of black. This pattern is in the cuticle and not in structures beneath it. At 30° *honey* has the same color as wild type raised at a tem-

perature high enough to prevent the formation of black in the body.

Study of unstained sections of the cuticle developed at 19° discloses no black pigment although areas in the praescutum over muscle masses are very dark. Changes in honey at lower temperatures seem to be due to an increased action of enzyme I, uninfluenced by II in any way for the lighter parts of the head and body lighten but little if any and the darker parts become red. Enzyme II, if present, has no visible effect. *Honey* may therefore be described as due to absence or extreme reduction of enzyme II.

Lemon

In a fraternity of one hundred twenty four males bred from an unmated daughter of a female crossed to an x-rayed male Miss Jane Maxwell found a mutant with lemon body color. It seems unlikely that this was caused by the treatment since in that case it should have occurred in 50 per cent of the fraternity. The trait is a striking one, ground color being pale lemon yellow, in contrast to the honey yellow characteristic of wild type and *honey*. Black pigmentation of the extremities, feet, wings and ovipositor sheath, resembles wild type but the antennæ differ in having the two basal segments, scape and pedicel, a clear yellow. *Lemon* is partially dominant to wild type. Pale basal segments and characteristic *lemon* praescutal pattern (described below) are dominant whereas general body color of the heterozygote resembles wild type.

Lemon as it appears at 30° and 19° is shown in Pls. IV G and IV H respectively. It will be seen from these illustrations that there are present on the præscutum two distinct anterior bands sharply divided by a light line, and, below these, irregular spots, often asymmetrical. Study of the cuticle of the *lemon* thorax by means of sections and removal from tissue underneath shows that, while the black pattern is in the cuticle itself, the spotted effect is due to structures below showing through the transparent portions which correspond to yellow

regions in the wild type and other mutant forms. The two præscutal bands characteristic of *lemon* outline more accurately the muscle masses beneath than does the solid patch of *non-lemon* forms, for dissection reveals a definite longitudinal division in the muscle masses of this region in all color types. This dividing line in the præscutal pattern is mentioned by Schlottke as appearing in some wild type individuals at high temperature and it sometimes shows in *honey* as a darker line in the center of the median præscutal patch but in *lemon* it is consistently present and more extreme.

Lower temperatures change the black pigment of the *lemon* mutant less strikingly than that of wild type or *black*. The black areas never become as intense or widespread but in spite of this there is a striking lightening of yellow. This fact taken together with the lighter general color at higher temperatures and the observed transparency of thoracic cuticle at both temperatures suggests a deficiency of enzyme I. There is too little of I to combine with II to form as much black as in wild type and likewise too little to produce the full amount of yellow even at high temperatures.

Double Recessives

Honey Black

In considering *honey black* from the theoretical aspect we might expect any one of three things to occur depending on whether increase of enzyme II associated with the factor *black* is less than, equal to, or greater than loss of II in *honey*. Since no black is developed in *honey* under any conditions tested and since *black* and wild type are so similar at low temperatures under which they produce the maximum amount of black the obvious guess would be that more of II has been lost in *honey* than is added in *black*. At any given temperature then, *honey black* would be expected to be lighter than wild type and darker than *honey* in respect to black for it would have the algebraic sum of the amounts of enzyme II associated with *honey* and with *black*. Pl. V 1 demonstrates that this is the case. Antennæ, wings and feet are not black-

ened but darker parts are brown. The presence of the *black* factor can be recognized in two ways, yellow areas are lighter than in *honey* and tips of the mandibles are black. Sections of cuticle of *honey black* reared at 19° show some dense pigmentation which is not as opaque as is found in forms without the *honey* factor. It is difficult to state with assurance whether this is black or a very dark brown. Pl. V J indicates that even at 19° no genuinely black spots develop although a very dark cast is apparent on all the brown areas and yellow areas are lighter than at 30°.

Lemon Black

Lemon black theoretically should have less of enzyme I and more of II than wild type. This would permit the production of black but at the expense of the reduced yellow which should be very pale. Pls. VI K and VI L picture what actually happens at 30° and 19° respectively. The *black* factor darkens the pedicel at 30°, both scape and pedicel at 19°. At both temperatures there is more black than in *lemon* and decidedly less yellow. In sections of wasps reared at 19° no portions of the cuticle can be identified as yellow. Black is diffused as a kind of sootiness over the animal (due in part to a darkening of the hairs) and this characterizes the behavior of mutant *black* in combination with other color factors. Evidently with a constant but reduced amount of enzyme I, an increase of II causes the production of more black, in other words more black is formed in *lemon black* than in *lemon* although no more of I is present.

Præscutum shows the typical pattern of *lemon*. This is due to the extreme reduction of yellow in the cuticle. *Lemon* pattern cannot be described as dominant over that of *black* for the factors are not allelic and the males pictured are haplonts. The *lemon* pattern may be said to mask the *black* pattern.

Lemon Honey

Lemon honey should have the reduced enzyme I of *lemon*, the reduced II of *honey*, less of both enzymes than wild type.

If the presence of II can be demonstrated in it, we must admit that some II is also in *honey*, which, although subliminal there, becomes expressed in combination with reduced I. A study of *lemon honey* at 30° (Pl. VII M) gives no evidence of any action of I–II for no black pigment is visible. The yellow areas are the same as in *lemon* at 30° and *honey* has contributed its lack of black. In a study of *lemon honey* at 19°, however (Pl. VII N), we recognize at once familiar signs of the action of enzyme II. Yellow areas are lightened and a faint sootiness appears on the characteristic *lemon* pattern of the præscutum. Evidently here with the same amount of II as in *honey* but in combination with less of I a small quantity of black pigment is developed at the expense of the yellow.

Triple Recessive

Lemon Honey Black

In the triple recessive, *lemon honey black*, there should be expected the enzyme I condition of *lemon* (less than wild type) and the enzyme II condition of *honey black* (less than wild type also). Such an animal with both enzymes reduced would be pale with evidences of both colors but little of either. Pl. VIII O confirms the expectation. The animal is a pale yellow with a faint sootiness and has black feet and mandibular teeth. The *black* factor behaves as in *lemon black*, darkening the pedicel at 30°, scape and pedicel at 19°. *Lemon* pattern on the præscutum masks both *honey* and *black*, due to transparency of the cuticle. Pl. VIII P shows the consistent effect of lowered temperature on the enzyme relationship. The sootiness has become more obvious, the yellow less so.

Discussion

Diagram 1 is an attempt to express in simple form the maximum potentiality of each genetic type in respect to each enzyme. Wild type (+) is used as a standard. Quantity of enzyme I is represented on the left, of II on the right. Any environmental condition which lowers the expression of II increases the expression of I. The scheme takes for granted a

constant amount of enzyme I in a given genetic type at all temperatures, with temperature of rearing determining how much of it shall act alone and how much shall combine with II. The maximum amount of enzyme II may be present at all temperatures but prevented from expressing itself during the shortened life cycle at the higher, or it may be produced in

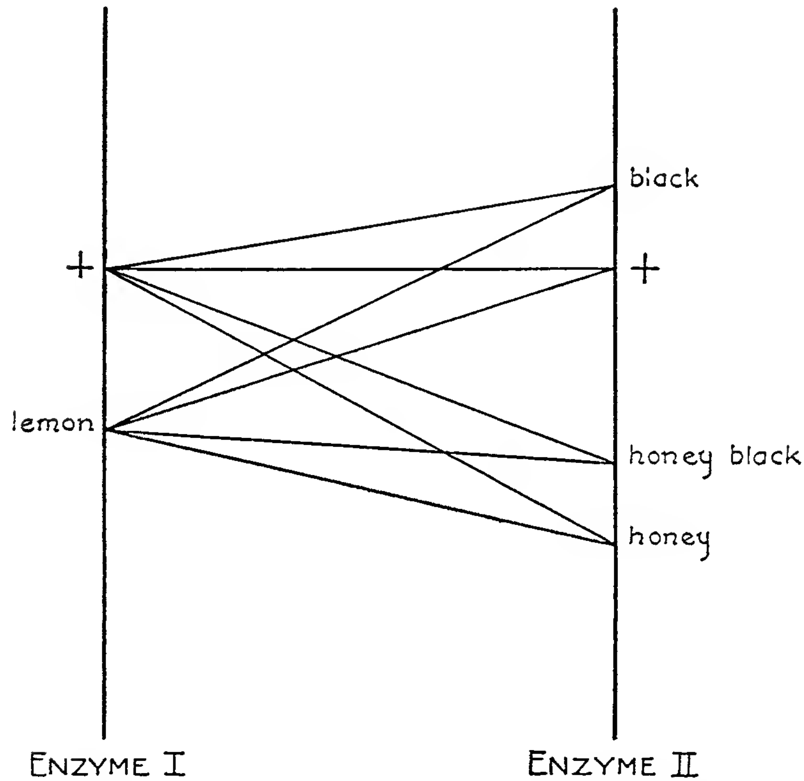

DIAGRAM 1. Relative potencies of enzymes I and II in each genetic type.

amounts determined by length of life cycle, indirectly through lower temperature. The behavior of I alone could be studied in relation to temperature only if a genetic type lacking all possibility of forming II were found. *Honey* most nearly approaches this but from its behavior with *lemon* we must conclude that it has at least a small amount of II. The levels of I therefore are established from the combined intensities of

yellow and black at any given temperature, those of II from the intensity and extent of black at 19°. The symbols at the ends of the lines are combined in identifying genetic types.

Effects of Humidity

At 30° wasps reared in the relatively dry air of the incubator and those in an atmosphere of 81 per cent relative humidity completed the life cycle in ten days. Throughout their development, however, one had the impression that those in the more moist air were slightly advanced beyond the others at any one time. If there were a difference it was so slight as to make no appreciable interval between time of emergence of the two groups as observed in this experiment.

With the two classes at 19°, on the other hand, those in an atmosphere of 81 per cent relative humidity were obviously more rapid in their development than those in one of 33 per cent. It could be observed that the former group was more advanced as larvæ and that they began to spin cocoons before the latter. They emerged five days earlier having required twenty two days to complete development. From this it might be concluded that increase in relative humidity increases speed of development, temperature remaining constant.

Maerks (1933) investigated the influence of temperature and relative humidity on the eggs of *Habrobracon*. He found the optimal temperature for hatching to be 29° C., the optimal relative humidity, 80 per cent. At optimal temperature eggs hatch almost simultaneously irrespective of relative humidity. The time required for hatching is increased by lower temperatures but is not noticeably influenced by relative humidity except at 19° and below when lower relative humidity slows up development (16°, 34 per cent, 127 hours; 76 per cent, 113 hours). At optimal temperature egg mortality is not significantly influenced by relative humidity; at low or high temperatures it is increased by low relative humidity; at optimal relative humidity it is low through a wide range of temperatures (16°–35°) but rises quickly to 100 per cent near the lower and upper limits (12° and 38°).

In the experiment here discussed eggs in jars *A* (30°, 33–39 per cent) and *B* (30°, 81 per cent) were at approximately optimal temperature; those in *B* and *D* (19°, 81 per cent) at optimal relative humidity; those in *B* at both and in *C* (19°, 33 per cent) at neither. *A* and *B* should behave similarly since relative humidity is of no importance at optimum temperature. *C* and *D* should, of course, require a much longer time for development than *A* and *B* because of lower temperature. There remains to be discussed the five day difference in length of life cycle between *C* and *D*. From comparison with Maerks' results eggs in *D* should have hatched about ten hours earlier than in *C*, and *D* mortality would be 3 per cent, *C* mortality 15–20 per cent (with the cold room at times somewhat below 19°).

Additional factors must be found to explain the five day lengthening of the life cycle in *C* besides the ten hour delay in hatching. Two possibilities remain. Lower relative humidity may have lengthened other stages as well as the egg in *C* or lower egg mortality in *D* may have resulted in less food per individual causing, perhaps, earlier spinning and pupation as observed in this group. The adults would then be smaller and, therefore, darker.

Study indicates that individuals from jar *D* are smaller than from jar *C* but it is difficult to say anything very definite about pigment differences since the two groups are so much the same in respect to pattern and intensity. Schlottke's method of classification was attempted but with no conclusive results except in the genetically *black* where there appears to be a consistent difference between the two groups, those from *C* having a small amount of yellow on præscutum (except in smaller individuals), those from *D* being entirely black. If, then, the speeding up of development in *D* tends to lighten the animals, this is counterbalanced by their smaller size.

Since this study was undertaken primarily with the object of obtaining adults for comparative study no detailed data, such as number of progeny per vial or per day, exact hour of hatching or emergence, etc., were kept.

Mosaics

In *Habrobracon* most sons of mated females and all sons of unmated are developed from unfertilized eggs and are haploid. The mosaic sons which occasionally appear among the progeny of unmated as well as mated heterozygous females must then have derived the various traits which they show from their mothers. Evidence indicates that the two nuclei differing in genetic makeup which enter into their formation are the egg pronucleus and the second polar body.

In the majority of eye color mosaics in *Habrobracon* (Whiting, A. R., 1934) each genetically different area is autonomous, separated from the adjoining one by a sharp line following facet boundaries. In eyes mosaic for any two of the allels in the orange series, however (wild type, dahlia, orange and ivory), there is interaction, a gradual shading from the darker to the lighter part with no clear boundary to indicate where cells of one genetic type stop and the others begin.

After the discovery of the body color mutants, their mosaics were awaited with much interest. Among progeny of matings made for linkage tests twenty-seven have been found by several different investigators. They include eight of the twenty eight combinations of body colors which might appear in mosaics. They are wild type and *honey* (11), wild type and *black* (1), wild type and *lemon* (2), wild type and *lemon honey* (4), *lemon* and *honey* (3), *black* and *lemon honey* (1) (Pl. IX Q), *lemon* and *honey black* (2), *lemon* and *lemon honey* (3) (Pl. IX R). In all combinations the genetically different regions are strictly autonomous.

Since each mosaic male begins development with two dissimilar nuclei, it might be expected that the descendents of each of these would make up about 50 per cent of the adult body. As far as may be judged from outside appearance this is not always the case. Occasionally a perfectly bilateral mosaic is found such as that illustrated by Pl. IX Q and, where the genetically different areas are scattered (Pl. IX R), they undoubtedly average 50 per cent each in some cases.

Exceptions to this are common, however, as illustrated by antero-posterior mosaics where the head may be of one type, the body and its appendages of another. There appears to be no regular order of distribution of the cleavage nuclei in the embryonic syncytium characteristic of insects.

Mosaics No. 813 and No. 824 have been selected for illustration and description. No. 813, Pl. IX Q, was found by Miss Gail Stocker. It is the type which causes the observer unfamiliar with insect embryology to remark that the traits must have segregated in the first cleavage so that each half has been developed from a separate blastomere. The mother of this mosaic was heterozygous for four linked factors, receiving *black* from one parent, *lemon, cantaloup* and *honey* from the other. It is obvious that they had segregated in this case as they had entered, without crossing over, as the left side is *lemon cantaloup honey*, the right *black*. The greater bulge characteristic of *cantaloup* is to be observed in the left eye.

Ventrally the insect is just as striking with a clear cut division extending through the head, thorax and abdomen. Genitalia are divided. The only exceptions to complete bilateral asymmetry are the mandibles and maxillæ which are *lemon honey*. The right half of the labium with its palp is *black*.

In striking contrast to this stands No. 824, Pl. IX R. The mother of this mosaic was wild type heterozygous for the linked genes *lemon, cantaloup* and *honey*, which had entered together. From the appearance it is impossible to say how the *cantaloup* factor is combined but the body areas are *lemon* and *lemon honey*, the *lemon* chromosome representing a cross over. Eyes are both mixed, left antenna *lemon*, right *lemon honey*. The left primary wing is *lemon* with a *lemon honey* spot, the remaining wings *lemon honey*. The first and third left legs are mixed, the second is *lemon*. The first right leg is *lemon*, the others *lemon honey*. Thorax is mixed on both dorsal and ventral sides. The remaining parts of the body are very light and difficult to classify with certainty.

Summary

1. Males of wild type, of the three linked body color mutant types, *black*, *honey* and *lemon*, and the four combinations of these reared under different conditions of temperature and humidity, are described.

2. More black pigment is produced at lower temperatures in all types except *honey*.

3. Increase of black pigment due either to environmental or to genetic factors is accompanied by decrease in intensity of yellow.

4. Higher relative humidity increases speed of development, temperature being constant.

5. *Black* and *honey* are recessive to wild type. *Lemon* is semidominant. Its light basal antennal segments and præscutal pattern dominate wild type in heterozygotes and mask the corresponding patterns of *honey* and *black* in double and triple recessives.

6. Wright's theory of pigment formation in mammalian hair and its apparent application to the cuticular pigments of *Habrobracon* are discussed.

7. Body color mosaics are described. In all combinations obtained each region is autonomous.

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EXPLANATION OF PLATES

Dorsal view of males of various genetic types reared at 30° C., 33–39 per cent relative humidity and 19° C., 33 per cent relative humidity. Magnification ×22.

PLATE I A. Wild type, 30° C.
PLATE I B. Wild type, 19° C.
PLATE II C. Black, 30° C.
PLATE II D. Black, 19° C.
PLATE III E. Honey, 30° C.
PLATE III F. Honey, 19° C.
PLATE IV G. Lemon, 30° C.
PLATE IV H. Lemon, 19° C.
PLATE V I. Honey black, 30° C.
PLATE V J. Honey black, 19° C.
PLATE VI K. Lemon black, 30° C.
PLATE VI L. Lemon black, 19° C.
PLATE VII M. Lemon honey, 30° C.
PLATE VII N. Lemon honey, 19° C.
PLATE VIII O. Lemon honey black, 30° C.
PLATE VIII P. Lemon honey black, 19° C.
PLATE IX Q. Male mosaic for black and lemon cantaloup honey, 30° C.
PLATE IX R. Male mosaic for cantaloup and for lemon and lemon honey, 30° C.

PLATE I A

PLATE I B

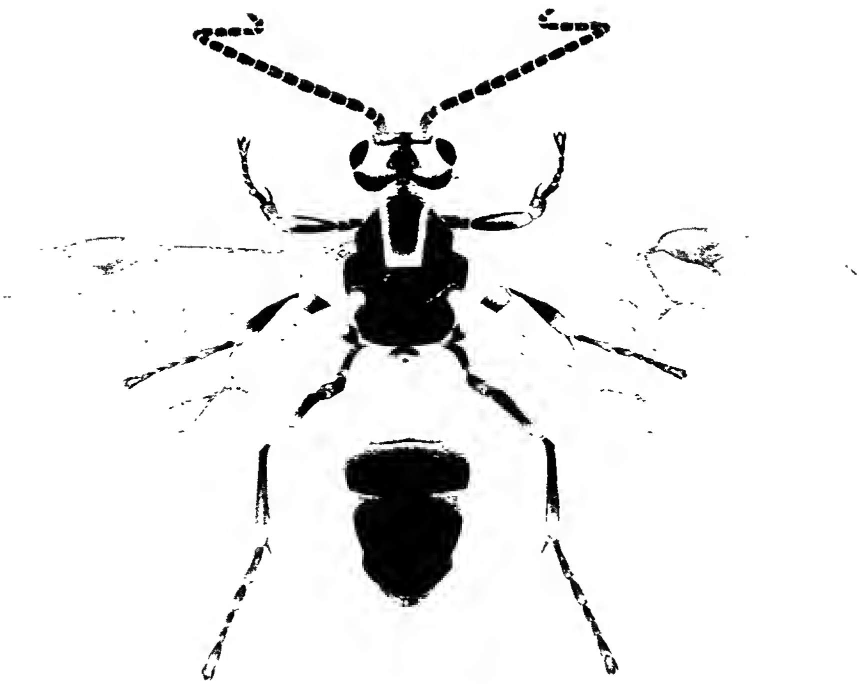


PLATE II c

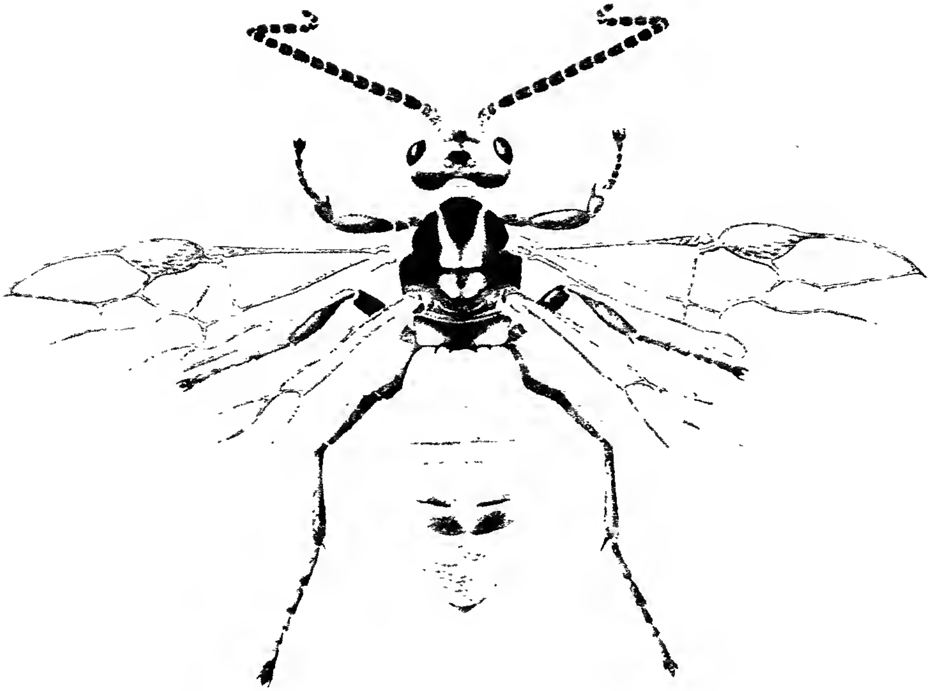


PLATE II D

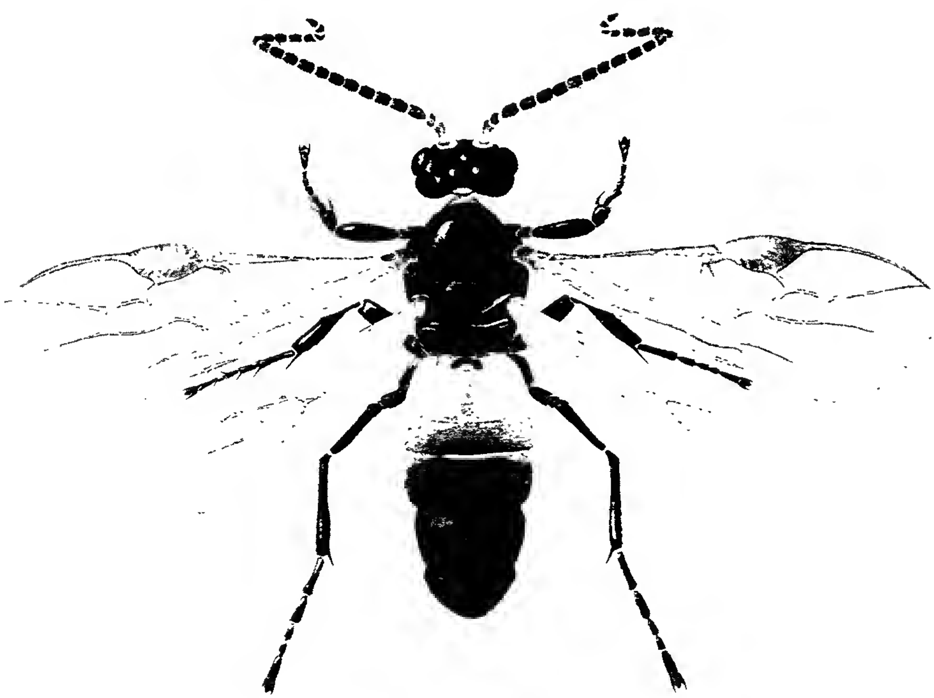


PLATE III E

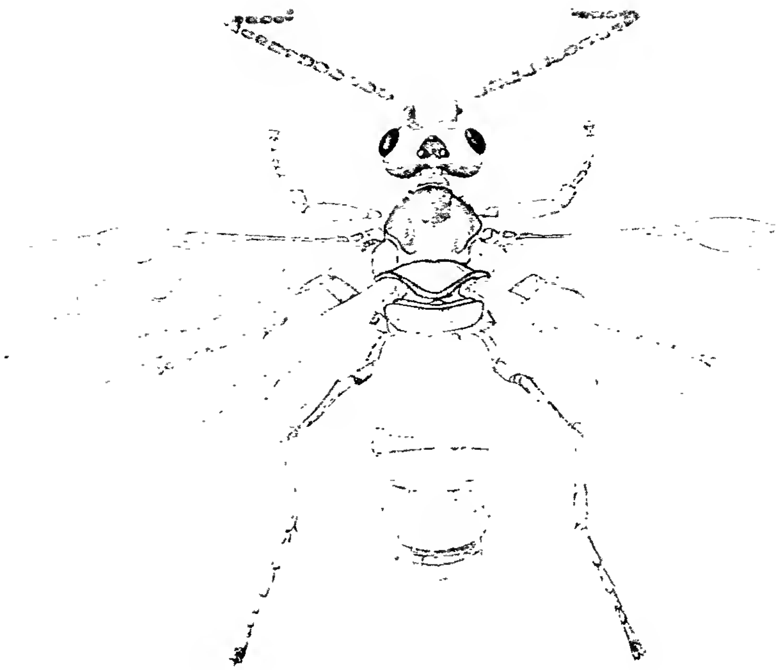


PLATE III F

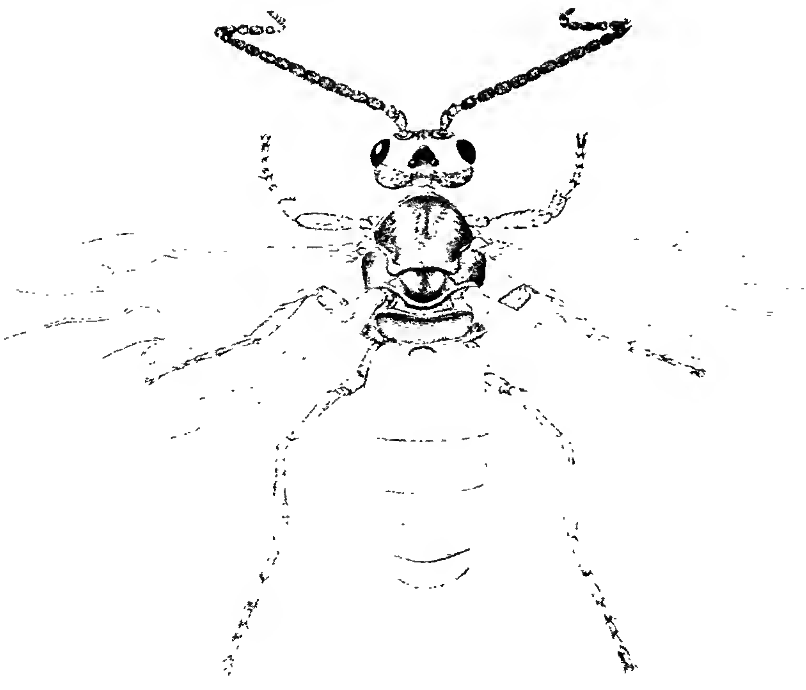


PLATE IV G

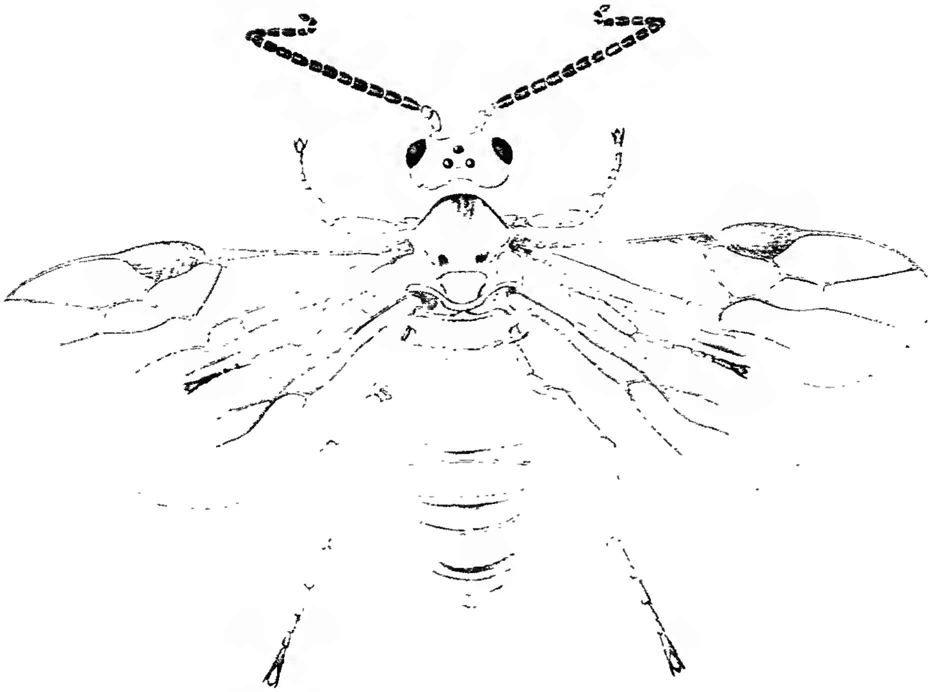


PLATE IV H

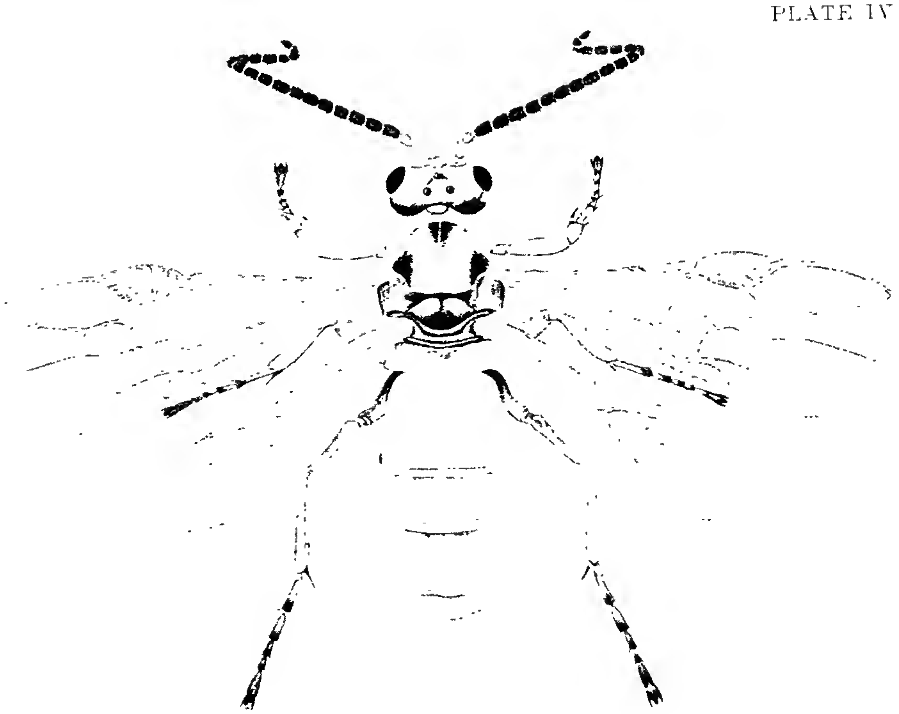


PLATE VI

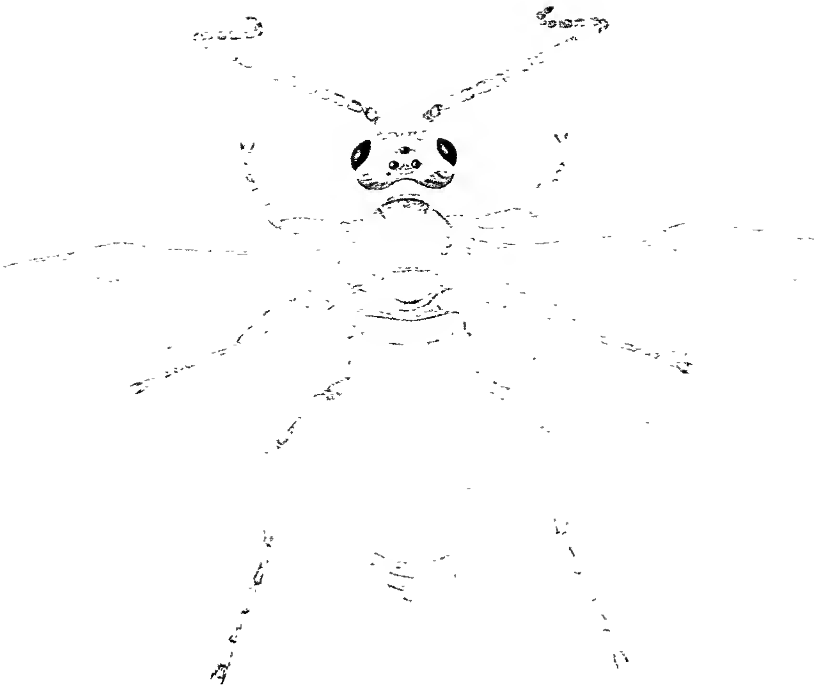


PLATE VI

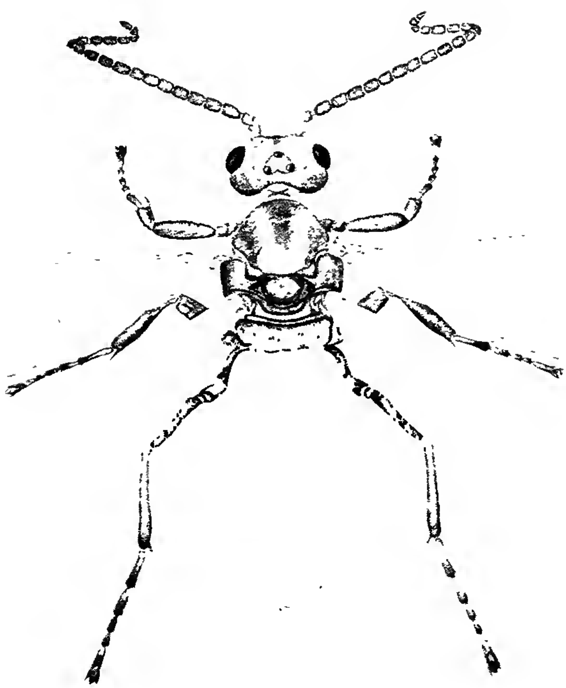


PLATE VI K

PLATE VI L

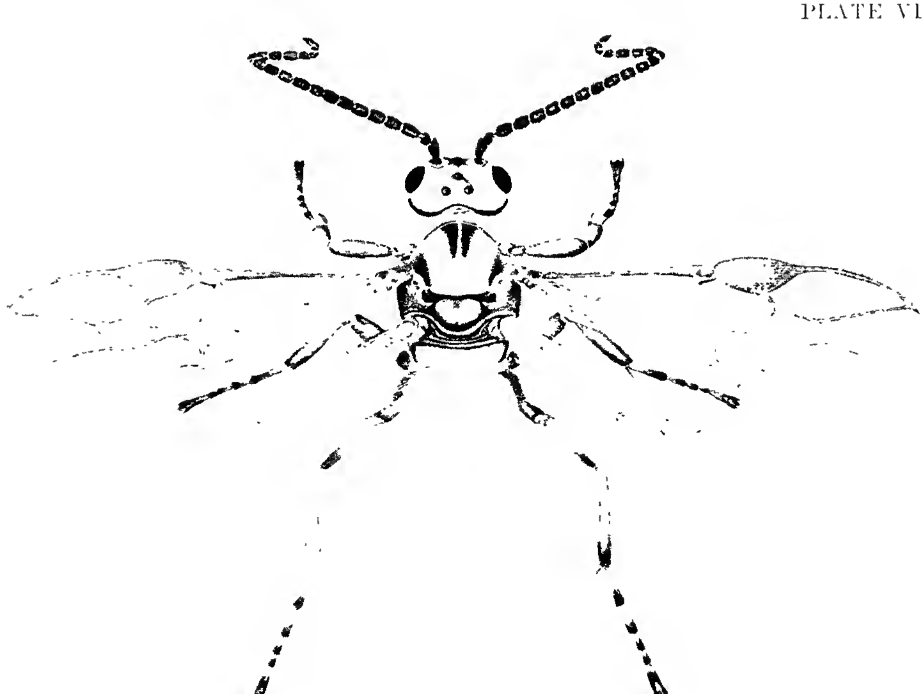
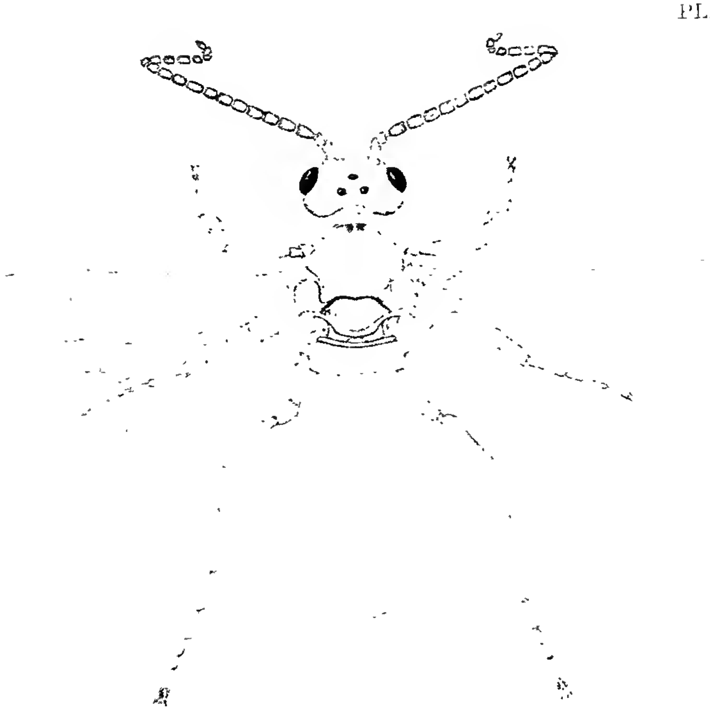


PLATE VII M

PLATE VII N



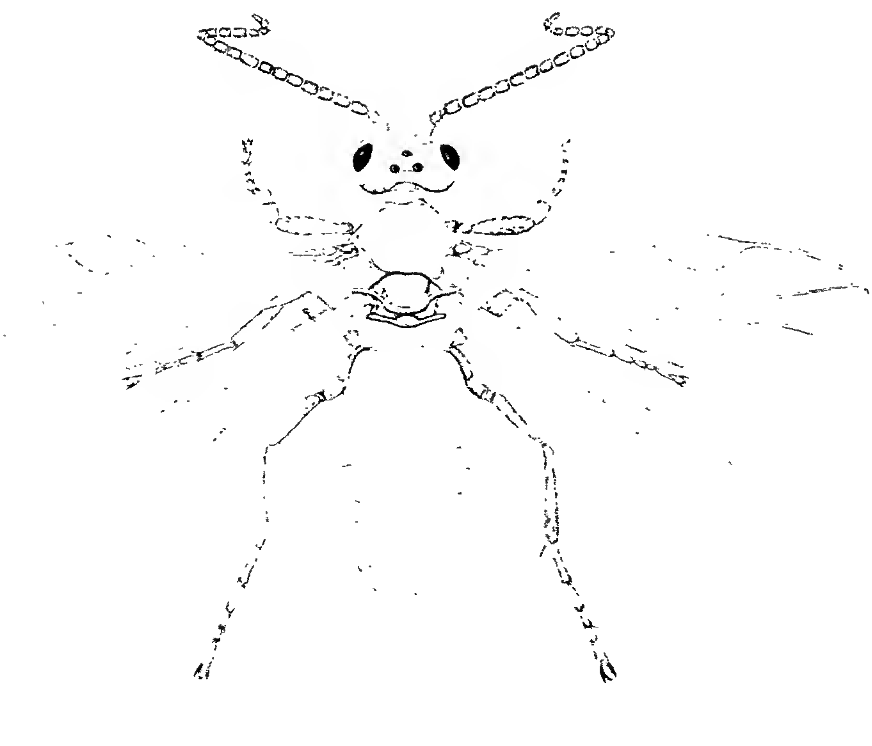


PLATE IX Q

PLATE IX R

MORE COMPLETE REMAINS OF OPHIURA MARYLANDICA

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(Communicated by Edward W. Berry)

ABSTRACT

Four years ago the author described a new species of *Ophiura marylandica* from disarticulated plates found in the Upper Miocene deposits of Maryland. In this original work a reconstruction of the animal was attempted which was correct in its diagnostic characters as has been proven by subsequent finds of complete specimens.

These complete *O. marylandica* were found preserved inside the large gastropods, *Fulgar coronatum*, *Ecphora quadricostata* and *Buccinofusus parilis*. Portions of twenty-five disks were discovered in ten shells, of which twenty-one exhibited the dorsal side while only four showed the ventral surface. It seems evident from a study of these shells that the animals must have, to a certain extent, entered the shells of their own accord and later became buried.

The fact that Ophiurans are not common as fossils is because the paleontologists have overlooked them and not because of any scarcity of the animals themselves.

IN 1934[1] the author described a new species of Ophiuroidea, *Ophiura marylandica*, from the Miocene of Maryland. This species was based upon disarticulated plates found in the unconsolidated sediments of the St. Mary's formation, from the region south of Cove Point, Calvert County, Md.

During the summer of 1937 several complete or nearly complete specimens of Ophiurans were reported as having been found in the sand filling the shells of *Fulgar coronatum* Conrad collected from the Miocene south of Cove Point. Upon hearing this the author made a trip to this locality in the fall of that year and collected all available *F. coronatum* Conrad as well as the other large gastropods in the hope that some might contain the remains of Ophiurans. In this he was rewarded for in one shell there were pre-

[1] Berry, C. T., "Miocene and Recent Ophiuran Skeletons," Johns Hopkins Univ. Studies Geol. no. 11, pp. 9–136, 1934.

served innumerable portions of arms and broken disks as well as the nearly perfect specimen which is illustrated in Fig. 2 of Pl. I.

Both Dr. R. E. L. Collins and Miss Schoonover have collected large gastropods from the St. Mary's formation south of Cove Point, Md., that contained Ophiurans. The author is deeply indebted to both for the privilege of examining and studying their specimens. A single specimen consisting of the greater portion of the outer lip of a *Fulgar* shell, probably *F. coronatum* Conrad, filled with a dark blue sandy clay was collected by Dr. Collins. In this clay it has been possible to distinguish the disks of seven specimens of *O. marylandica,* and innumerable fragments of arms belonging to the same species. None of these Ophiurans are as perfect as the one illustrated. The diameter of their disks varies from approximately 5 mm to 11 mm.

The material collected by Miss Schoonover represents a much larger collection consisting of one *Ecphora quadricostata* (Say) one *Buccinofusus parilis* Conrad and six *Fulgar coronatum* Conrad as well as portions of the outer lip of the same species of *Fulgar.* All of this material was carefully examined and as a result portions of seventeen disks of *Ophiura marylandica* were uncovered as well as innumerable fragments of arms. In thirteen cases the dorsal side of the disk was uppermost, some with the dorsal disk plates present while others had been lost. In the latter instances the vascular region was exposed. Only four cases were found which showed portions of the ventral side of the disk. None of the seventeen disks were as perfect as the one in Fig. 2 of Pl. I; all showed some warping or distortion.

Ophiura marylandica C. Berry

Pl. I, Figs. 1, 2, 3

Dorsal side Pl. I, Fig. 2.

This specimen was collected by the author on Oct. 9, 1937, about 3½ to 4 miles south of Cove Point, Maryland,

from the St. Mary's member of the Miocene formation. The Ophiuran is situated in the upper central portion of the aperture of *Fulgar coronatum* Conrad. The dorsal side of the disk is exposed with three arms descending into the sand, which were not entirely uncovered. Another arm extends dorsally at right angles to the disk, while a fifth extends laterally for a distance of 11 segments to where it is broken. In the center of the disk is the central disk plate which is pentangulate in outline, the sides being interradial in position. This central disk plate overlies five small plates abutting on each side. These in turn overlie five large plates (about twice the size of the preceding ones). These large plates are also separated by smaller ones and overlie a series of small plates. From this center rosette there radiates ten rays, five of which are embraced by the pairs of radial shields and five between the respective pairs. The rays between the right and left radial shields are composed of two plates, being longer than broad. The more central one is the longer while the outer one is long and thin, the outer end of it being somewhat pointed. The rays between adjacent pairs of radial shields are composed of a number of plates. There are three major ones all of which are longer than broad, while the one near the margin is nearly circular. These plates overlap a number of long narrow ones. Due to the condition of the specimen these rays are not perfectly preserved.

The interradial marginal plates are surrounded on the dorsal side by seven small plates, one in the center and three at each corner, occupying the space between the interradial marginal plate and the comb plate. The comb papillæ were not preserved intact on any of the comb plates. The first, second, and third dorsal plates are flanked on either side by the comb plates. The diameter of the disk is 12 mm. No evidence was found that the disk was covered by fine calcareous granules which during life would have hidden portions of the plate. This is true of the

specimen figured and all the subsequent ones so far examined.

Several segments of the arm which extends at right angles to the disk of the Ophiuran were carefully studied and compared with the disarticulated plates found in the same shell. All these plates resemble one another, showing without a doubt that more than one specimen of the same species was trapped in this shell. Thus one concludes that the ventral disk plates which are also present belong to the same species. All these plates compare exactly with those described in 1934 proving beyond contradiction that this complete find is a specimen of *Ophiura marylandica*. It is regrettable that the reverse side of the disk shown in Pl. I, Fig. 2 cannot be viewed, but the fragile condition of the specimen—even after hardening—does not permit this side to be exposed.

Ventral side, Pl. I, Figs. 1, 3.

The two specimens described below were among those collected by Miss Schoonover. In the first specimen only three-fifths of the ventral surface is preserved, consisting of three oral shields, portions of the like number of arms, one interradial marginal plate, six side mouth shields, three sets of jaws, and numerous small ventral disk plates.

The side mouth shields flank the adoral end of the oral shields, while protruding from below the former are the adoral ends of the jaws. Spanning the adoral ends of these jaws are the torus angularis, all of which are lacking teeth. The first five ventral arm plates are present on each of the three arm fragments, while the ventral ends of the lateral arm plates can only be discerned in two of the arms, in one of which the tentacle pores can be clearly seen. The small ventral disk plates that are located in the aboral region of the oral shield can only be seen in one section and here they are distorted to such an extent that their original arrangement cannot be discerned. The ventral side of the interradial marginal plate is present.

The second specimen consisting of the fragments of two arms, one oral shield, two side mouth shields, and one pair of jaws shows these small disk plates in question. At the center of the aboral end of the oral shield is a small circular plate partly overlying and flanked by two larger circular ones which are at the aboral corners of the oral shield. Between the small central plate and the interradial marginal plate there is a long semi-oval centrally located plate. At the ends of this plate are two rounded plates, which are also at the ventral corners of the interradial marginal plate. In the ventral region on each lateral side of the interradial marginal plates there is a large oval plate. In the space between the arms and the small plates already described are a number of small plates of which the exact shape and number cannot be determined. However they appear to be successively overlapped by the ones nearer the central region.

Innumerable tentacle papillæ, mouth papillæ, teeth and arm spines were observed in the sediment in the region of the disks and arms in practically every specimen examined. None of these small plates adhered to the larger ones so that their number could not be counted in reference to a specific given region. Those which were in immediate proximity to the larger plates became disarranged in uncovering the specimen. This condition, though to be expected, is regrettable, for these minute plates are important in the identification of living Ophiurans.

In the original description of the species the statement was made[2] that the arms of *O. marylandica* were composed of from 45 to 60 segments. The recent finds shows that this estimated length was too great and that in all probability the arms did not exceed 40 segments except in especially large individuals. In order to obtain as accurately as possible the size of these fossil animals the arms of two specimens which Miss Schoonover had collected were carefully studied and in one it was found that the arm was com-

[2] Berry, C. T., *op. cit.*, p. 29, 1934.

posed of 24 segments which decreased in size from 2.24 mm across at the margin of the disk to 1.20 mm in the last segment preserved. The diameter of this disk was approximately 13 mm. In another specimen from the same collection the disk of which was 8.50 mm in diameter it was found that its arm decreased from 1.84 mm at the disk to 0.56 mm at the 26th segment. The two living species, *Ophiura sarsii* and *O. texturata*, nearest to this fossil one, had arms which ranged up to 60 segments in length, the average being however about 50 segments. This clearly emphasizes the smallness of the Miocene species.

The question arises as to why and how these animals were trapped in the shells. This can partly, but not entirely, be explained by the fact that Ophiurans are naturally nocturnal and accustomed to hide under rocks and in shells during the day. Several additional explanations present themselves, among which are the following outstanding ones. First, that the animals died elsewhere and were washed in. Second, the animals died in the shells and were covered up. Third, they were buried alive in the shell by sedimentation.

The first of these explanations can be quickly disproven for if such were the case the arms would not be attached to the disks, but would have been broken off as the dead animal was rolled along the sea bottom. However, many of the disarticulated individual plates which are found scattered in the material must have been brought in by this method, even though they do not show a great amount of abrasion.

The second case is questionable, for if the animal had died in the shell the rate of normal sedimentation would have been slow enough to allow it to have become dismembered before covering. Certainly this could not explain a specimen as shown in Pl. I, Fig. 2. However, if an abnormal condition prevailed with reference to sedimentation and it was speeded up to such a rate that the animal was quickly covered after death, the possibility of finding such

a perfect specimen would not be out of the question. But did this condition of sedimentation exist at this time and was it local or wide spread?

Thirdly, in all probability the animal would not have allowed itself to be buried alive under normal conditions. Thus it is necessary to assume that there were possibly ab-

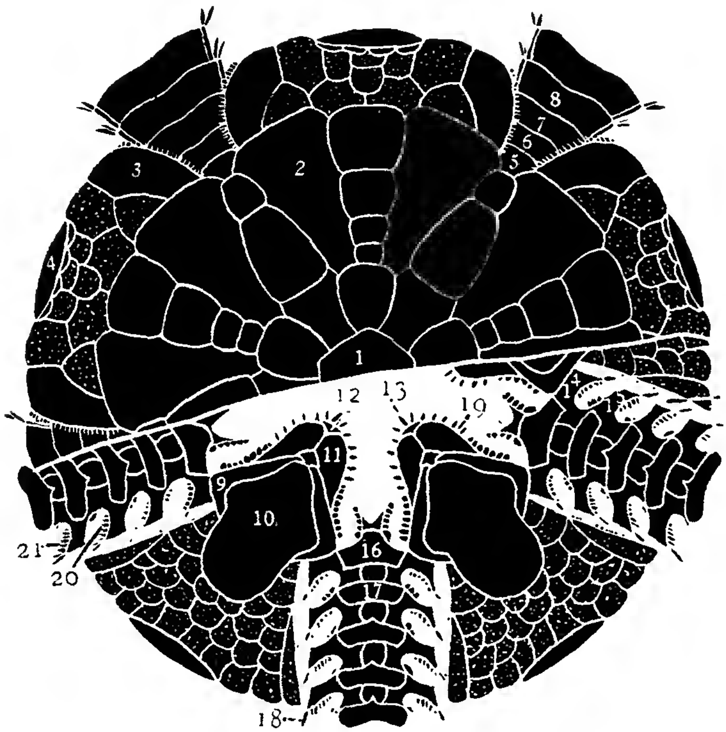


FIG. 1. Reconstructed *Ophiura marylandica*. The upper half of the figure shows the dorsal side, while the lower half shows the ventral side. The stippled plates are hypothetical.

normal local conditions like strong wave agitation which would disturb the shallow Miocene sea, thus sweeping great amounts of material into the shell occupied by the hiding Ophiurans.

There appears to be no general arrangement of the Ophiurans in the shells. Sometimes the individuals will be

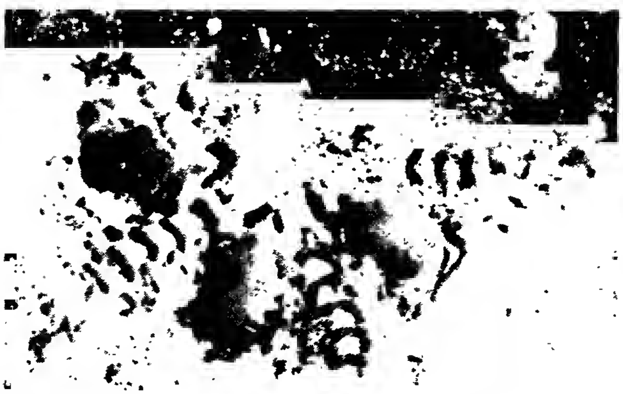
at right angles to each other, very often being mixed in with the great number of pelecypods which fill these gastropod shells. The fact that a great number are found in a single shell points to their gregarious habits. There is probably no single cause to which the presence of these Ophiurans in gastropod shells can be attributed, but to many working together at various times. Whether or not those gastropod shells which contain Ophiurans are from the same zone cannot be ascertained for no such information was obtained at the time they were collected. However, in 1934 it was found that the disarticulated plates had a vertical range of 15 feet.

When the author first described *Ophiura marylandica* basing this species on disarticulated plates, he attempted a reconstruction of this species as reproduced in Fig. 1; Figs. 1, 2 and 3 of Pl. I are the actual specimen of *O. marylandica* found three years after it was first described. A close scrutiny of these figures will show that only a slight difference exists between the actual specimens and the restoration. This difference lies in the small secondary plates and not the large primary ones, and in the portion of the disk occupied by both. Such a similarity between a reconstruction from disarticulated plates and the actual specimen proves conclusively the practicability of the work on the disarticulated plates, which are common in many of the unconsoliated Tertiary sediments. This finding of the Ophiurans in the gastropod shells is probably not such a chance case of preservation as it may seem, but is probably a common occurrence, and can be duplicated now that their actual presence inside shells is to be expected.

PLATE I

1

2



3

Fig. 1. Ventral side of *Ophiura marylandica*. $\times 5$.
Fig. 2. Dorsal side of *O. marylandica*. $\times 3\frac{1}{4}$.
Fig. 3. Ventral side of *O. marylandica*. $\times 5$.

ARCHÆOLOGY AND ASTRONOMY

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(Read April 23, 1938)

ABSTRACT

The following article is an attempt to illustrate a method of obtaining more accurate information concerning the dates of Greek temples and certain details of religious practice through the application of an outmoded theory, that of "orientation." During the last thirty years of the nineteenth century, the theory that Greek temples (of which the majority face approximately eastward) were erected with reference to the direction of sunrise on the foundation day, was widely accepted and numerous observations were accumulated. The astronomical calculations based on the data, however, led in most cases to such fantastic dates for the temples that more recent opinion has tended to discredit or ignore the theory. But the fragments of literary evidence and the accumulated measurements of the actual orientations, taken together, furnish sufficient proof of the existence of the general principle. The following article endeavors to show that the error lay, not in the theory itself, but in the deduction of results from incomplete data. In the absence of detailed knowledge of the archæological conditions, of the religious festivals, and of the various local lunar calendars, investigators felt obliged to check the observed orientations with reference to assumed astronomical factors such as the heliacal risings of stars—factors which could never have been employed by peoples observing lunar calendars—with results inevitably grotesque. During the last few decades, however, our knowledge of the true governing factors, the archæological and historical conditions, the identity of the divinities worshipped in the temples and the festivals connected therewith, and the ancient calendars, has been steadily increasing. In the following pages this accumulated information is applied to a series of Greek temples, for the purpose of rehabilitating the theory of orientation and of deriving from it additional knowledge.

THE reader hardly requires to be informed that I know nothing of astronomy. This fact, if not already self-evident, will be obvious before many moments have elapsed. My only excuse for intruding upon such a field is the precedent established by Sir Norman Lockyer, who wrote upon archæology from the astronomer's point of view, though his archæology, like my astronomy, was purely empirical. But I venture to hope that my ignorance of this subject is not an extreme disadvantage; in other words, it is possible that my primitive

state of mind may place me closer to the ancients with whom I am concerned, and that it may restrain me from the temptation to overshoot the mark.

In short, I wish to investigate a subject already sufficiently discredited, the astronomical orientation of ancient temples, and specifically of Greek temples.[1]

The direction assumed by a building may be determined by one of five factors: (1) pure chance, (2) topography (relation to ground, streets, or other buildings), (3) climate (directions of sun, prevailing winds, considerations of health), (4) religion (directions prescribed by cult or sanctified by tradition), or (5) a blind devotion to astronomy. Some of these factors appear side by side in a map of modern Athens. Most of the modern buildings are located on the basis of topography, an artificial topography, to be sure, created by a city plan of a century ago. The ancient buildings on the Acropolis are placed in accordance with religious demands. But the older buildings of the British and American Schools are unrelated to religion, climate, or topography; neither the slope of the ground nor the directions of the streets were considered. Pure chance dictated the position of the British School; a tennis-court haphazardly located furnished a ready-made cellar excavation. Blind devotion to astronomy fixed the direction of the British Director's residence: it points exactly east, toward the equinoctial sunrise, and the tradition thus established was so potent as to cause the erection of the American School, with its north face coinciding with the prolongation of the south line of the British School, so that the cardinal points of the compass were maintained. Both the British and American Directors were granted unobstructed views of the rising sun.

Having studied archæology in this astronomical environment, I am naturally inclined to investigate the extent to

[1] This paper, while not a report upon my activities as recipient of a grant from the Penrose Fund of the American Philosophical Society, owes its existence to a momentary experience in connection with the work undertaken under this grant at Athens in 1937, combined with random observations accumulated during the past twenty years.

which astronomical observations may have been utilized in placing buildings governed primarily by other factors. Such an investigation, of course, is not a new idea. The cromlech at Stonehenge in England has attracted attention for generations because its axis so closely approximates the direction of sunrise at the summer solstice. As early as 1771 Dr. John Smith argued that it was an astronomical temple, and measured its orientation as 40° north of east which he compared with the amplitude of sunrise at the solstice, without, however, attempting to calculate the date:[1] Wansey, in 1796, mentioned the precession of the equinoxes in connection with Stonehenge; and in 1827 Higgins used this phenomenon in an attempt to calculate its date, obtaining 4000 B.C.[2] In 1869 Nissen published his idea that the varying orientations of Greek and Roman temples might be due to the varying days on which their foundations had been laid out.[3] In 1872 Burnouf made a more detailed examination of the temples on the Athenian Acropolis from this point of view.[4] In 1880 came Petrie's new publication of Stonehenge, again estimating the date by precession, but obtaining 730 A.D.[5] In 1891 Sir Norman Lockyer began to publish a series of investigations applying the theory to the temples of Egypt,[6] and subsequently extended his research to the stone monuments of Great Britain.[7] Francis Penrose immediately adopted the

[1] Smith (J.), *Choir Gaur* (1771).

[2] Higgins (G.), *The Celtic Druids* (London, 1827).

[3] Nissen, *Das Templum, Antiquarische Untersuchungen* (Berlin, 1869). See also his later articles in *Rheinisches Museum*, XXVIII, 1873, pp. 513–557; XXIX, 1874, pp. 369–433; XL, 1885, pp. 38–65, 329–370, 480; XLII, 1887, pp. 28–61 (some of these revised in his final book, *Orientation*, 1906–1910).

[4] Burnouf, *La légende athénienne, étude de mythologie comparée* (Paris, 1872). *Cf.* Bötticher, *Die Tektonik der Hellenen* (2nd ed., 1874), II, pp. 517–523.

[5] Petrie, *Stonehenge—Plan, Description, and Theories* (London, 1880), pp. 18–20.

[6] Lockyer, in *Nature*, 1891, April 16, May 7 and 21, June 4, July 2; 1892, Jan. 28, Feb. 18; *Nineteenth Century*, 1892, July; *Dawn of Astronomy, A Study of the Temple-worship and Mythology of the Ancient Egyptians* (New York and London, 1894).

[7] Lockyer (with Penrose), in *Proc. Roy. Soc.*, LXIX, 1901/2, pp. 137–147, reprinted in *Journal Roy. Inst. British Architects*, IX, 1901/02, pp. 137–142; *Stonehenge and other British Stone Monuments Astronomically Considered* (London, 1906; 2nd ed., 1910); Griffith, in *Life and Work of Sir Norman Lockyer* (London, 1928), pp. 395–426.

theory with respect to Greek temples.[1] Nissen's final work summarized the facts and theories, especially in the classical field, as attained before 1906.[2] Since this final summary, comparatively little work has been done in this field, apart from additional surveys of British megalithic monuments,[3] and the extension of the study into Mesopotamia.[4]

On the other hand, the favor with which these theories were at first received has waned in recent years. Most investigators of the present day, particularly in the classical field, completely disregard them; and others oppose them as absurd or contrary to fact,[5] as irreconcilable with the requirements of history or archæology. My objection to such categorical statements is that the so-called facts of history and archæology are daily altering their appearance; in many pertinent cases our knowledge of to-day is very different from

[1] Penrose, in *Nature*, 1892, Feb. 25; *Proc. Roy. Soc.*, LIII, 1893, pp. 379–384; LXI, 1897, pp. 76–78; LXV, 1897/8, pp. 370–375; LXVIII, 1901, pp. 112–114; *Trans. Roy. Soc.*, CLXXXIV, 1893, pp. 805–834; CXC, 1897, pp. 43–65; CXCVI, 1901, pp. 389–395; *Bulletin de Correspondance hellénique*, 1896, pp. 383–385; 1900, pp. 611–614; and in *The Argive Heraeum*, I, pp. 28–29. *Cf.* Koldewey and Puchstein, *Die griechischen Tempel in Unteritalien und Sicilien* (Berlin, 1899), pp. 190–191.

[2] Nissen, *Orientation, Studien zur Geschichte der Religion* (Berlin, 1906–1910); also article in *Neue Jahrb. f. klass. Altert.*, 1908, pp. 219–240. Reviews of Nissen's work in *Rev. critique*, 1906, pp. 381–382 (Maspero) and 1908, pp. 81–82 (My); *Journal des Savants*, 1908, pp. 103–104 (Toutain); *Deutsche Literatur-Zeitung*, 1908, p. 1139; *Wochenschr. f. klass. Phil.*, 1907, pp. 228–232; 1908, pp. 257–261; 1910, pp. 1161–1165 (Ginzel); *Phil. Wochenschr.*, 1910, pp. 974–981 (Otto).

[3] Somerville, in *Journ. British Astron. Assn.*, 1912/13, pp. 83–96; *Man*, 1922, pp. 133–137; *Archaeologia*, 1922/23, pp. 193–224; *Antiquity*, 1927, pp. 31–41. Also Stone, *Man*, 1922, pp. 114–118; *Antiquaries Journal*, 1923, pp. 130–134; *Stones of Stonehenge* (London, 1924), pp. 20–30, 125–127, 129–134. Also Petrie, *Man*, 1924, p. 107; Trotter, *Antiquity*, 1927, pp. 42–53; Cunnington, *Stonehenge and its Date* (London, 1935), pp. 51–70, 87–88, 125.

[4] Martiny, *Die Kultrichtung in Mesopotamien* (Diss. Berlin, 1932). Dr. W. F. Albright has kindly brought to my attention a review of this by Neugebauer and Schott, *Zeitschrift f. Assyriologie*, XLII, 1934, pp. 198–217.

[5] Compare A. von Gerkan, *Griechische Studteanlagen* (Berlin, 1924), pp. 74–82, an adverse criticism on the classical side. Adverse concerning the megalithic monuments are Holmes (R.), *Ancient Britain and the Invasions of Julius Cæsar* (London, 1907), pp. 215–217, 468–477; *Antiquaries Journal*, 1922, pp. 344–349; Engleheart, *Antiquity*, 1930, pp. 340–346. For Mesopotamia, E. Unger bases the orientation rather on the directions of prevailing winds (in *Forsch. u. Fortschr.*, IV, 1928, pp. 343 ff.; V, 1929, pp. 270 ff.; *Babylon, die heilige Stadt*, pp. 122 ff.), but his conclusions are disputed by Martiny.

that of thirty, or even of ten years ago. It seems desirable to submit these facts to a fresh examination, and to ascertain whether they are in actuality so inapplicable to the astronomical conditions.

Why should a theory, in itself logical and worthy of examination, have been received with suspicion by modern scholars? Surely we have analogies enough, even in our Christian churches with their traditional east-and-west orientation, to justify such an inquiry. Nor are we limited to the monumental evidence. Running through the world's literature is a thin stream of documentary record. The Egyptian king, "the living God, the magnificent son of Thoth (Asti), nourished by the sublime goddess (Hathor) in the temple of the sovereign of the country, stretches the rope with joy. With his glance at the middle (? Ak) of the Bull's Thigh (Great Bear) [1] constellation, he establishes the temple-house of the Mistress of Dendera, as took place before." And then the king says, "Looking to the sky, at the course of the rising stars [and] recognizing the middle of the Bull's Thigh constellation, I establish the corners of the temple of Her Majesty." [2] Of course such statements, which are fairly numerous in the Egyptian inscriptions,[3] might refer to nothing more than the selection of a propitious moment, the motive being astrological, and the ropes being stretched by the *harpedonaptae* in directions quite different from those required by astronomical orientation.[4] Such an astrological motive was that of Filippo Strozzi, when he laid the corner-stone of his palace at Florence at the moment of sunrise on

[1] Wainwright, in *Studies Presented to F. Ll. Griffith*, pp. 375–383; *Journ. of Egyptian Archæology*, 1936, pp. 45–46.

[2] Quoted by Dümichen, *Baugeschichte des Dendera-Tempels* (1877), p. 30; Lockyer, *Dawn of Astronomy*, p. 176; Penrose, *Trans. Roy. Soc.*, CLXXXIV, 1893, p. 806; Nissen, *Orientation*, p. 33.

[3] For such inscriptions see Dümichen, *op. cit.*, pp. 32–33; Lockyer, *op. cit.*, pp. 173–181; Nissen, *op. cit.*, pp. 31–34; Hambidge, *Dynamic Symmetry*, p. 143.

[4] The skill of the rope-stretchers or *harpedonaptae* is implied by Democritus (quoted by Clement of Alexandria, *Strom.*, I, p. 304A): "no one has yet surpassed me in the construction of lines with demonstration; no, not even the Egyptian *harpedonaptae*, as they are called, with whom I lived five years in all, in a foreign land."

August 6, A.D. 1489;[1] but the palace itself faces almost exactly east, disagreeing with the northeasterly direction of sunrise on that day. Admitting, therefore, that we must be on our guard with respect to the Egyptian statements, we seem to reach more solid ground in the case of those of classical times. Plutarch (*Numa*, 14, 4) says that "the worshipper who enters a temple, since temples face the east and the sun, has his back towards the sunrise." And Lucian (*On the House*, 6) refers to "the eastern aspect, procuring us, as in the temples of old, that first welcome peep of the sun in his new-born glory, and suffering his rays to pass in without stint through the open doors." On the other hand, Vitruvius (IV, 5) and Clement of Alexandria (*Strom.*, VII, p. 724) claim that temples should face westward; the same idea is expressed by Frontinus (*Grom.*, p. 27).[2] The basic theory is after all, the same; as Vitruvius puts it, "the aspects which the sacred temples of the immortal gods ought to regard are to be so appointed (if no reason hinders, and the opportunity is presented) that the temple and the statue which is in the shrine look toward the western quarter of the sky, so that those who come to the altar to sacrifice or make offerings may look towards the eastern heaven and the image in the temple. . . . For all the altars of the gods should look to the east." We obviously have here the reflection of a divergent school of thought which seems to have been embodied in the lost writings of Vitruvius's master Hermogenes, a theory which never won approval. A reflection of the controvery appears in Hyginus (*Grom.*, p. 169): "The ancient architects (*i.e.* Vitruvius, etc.) wrote that temples should properly look toward the west; but afterwards it seemed good to every religion to change to the direction of the quarter of the sky from which the earth is illumined." With regard to the details of orientation, the architect Vitruvius tells us nothing,

[1] For foundation ceremonies of all periods, see Rowald, *Beiträge zur Geschichte der Grundsteinlegung*, in *Zeitschrift f. Bauwesen*, 1904, pp. 41–66, 271–288, 395–416 (for the Strozzi case, pp. 399–400).

[2] Lachmann, *Gromatici veteres* (1848).

either for temples or for towns. But the Roman field-surveyors (*agrimensores*), whose functions did not extend to the location of temples, give explicit information with regard to the related subject of the orientation of towns.[1] And here again we find two opposed practices. According to some, the orientation should be equinoctial, true east-and-west: thus Pliny (XVIII, 34) says of the *decumanus* axis, "this will go from equinoctial sunrise to equinoctial sunset." But others preferred to follow the daily variation of sunrise: Frontinus (p. 31) says that "many differed from this scheme by following the movable rising and setting of the sun; and thus it was so executed that the *decumanus* looked in the direction in which the sun made its appearance at the time that the measurement was made."[2] Obviously Vitruvius might have been just as explicit with regard to town planning, if he had so desired; the omission of the details of orientation in the case of towns suggests that he may have been equally reticent in the case of temples.

The survival of the tradition through the Middle Ages, while sufficiently obvious in the plans of the churches, which show the same divergence of opinion between easterly and westerly orientation that we encounter in antiquity, is attested also by a few passages in literature. With regard to specific information, however, it is with Christianity as with the Greeks and Romans: detailed facts are given only by the latest writers. In a work published in 1700, we read that "one end of every Church doth point to such Place, where the Sun did rise at the time the Foundation thereof was laid, which is the Reason why all Churches do not directly point to the East; for if the Foundation was laid in *June*, it pointed to the North-east, where the Sun rises at that time of the Year; if it was laid in the Spring or Autumn, it was directed full East; if in Winter, South-east; and by the standing of these

[1] On these writers, besides Lachmann (*op. cit.*), see Cantor, *Die römischen Agrimensoren und ihre Stellung in der Geschichte der Feldmesskunst* (Leipzig, 1875).

[2] For similar statements of the *agrimensores* see Frontinus (p. 108), Hyginus (pp. 170, 182, 183), also Isidore of Seville (*Orig.*, XV, 4, 7).

Churches, it is known at what time of the Year the Foundations of them were laid."[1] In similar vein is Wordsworth's poem "On seeing the Foundation preparing for the Erection of Rydal Chapel, Westmoreland," in 1823:

> Then to her Patron Saint a previous rite
> Resounded with deep swell and solemn close,
> Through unremitting vigils of the night,
> Till from his couch the wished-for Sun uprose.
>
> He rose, and straight—as by divine command—
> They, who had waited for that sign to trace
> Their work's foundation, gave with careful hand
> To the high altar its determined place.

As the poet himself remarked in explanation, "our churches, invariably perhaps, stand east and west, but why is by few persons exactly known, nor that the degree of deviation from due east often noticeable in the ancient ones was determined, in each particular case, by the point in the horizon, at which the sun rose upon the day of the saint to whom the church was dedicated."[2]

If we seek an explanation of the disrepute into which the study of orientation has now fallen, it must be attributed, I think, to the niceties of modern astronomical calculation. What would have been a simple process in antiquity, the mere observation of the point of rising or setting of the sun or, as some think, of a certain star, on a selected day in the then current year, must now be laboriously reconstructed. Yet it is now possible for us to estimate the minute changes resulting from the reduction in the obliquity of the ecliptic (0° 0′ 41″ per century),[3] and the greater changes brought about from the precession of the equinoxes (1° 23′ 32″ per century);[4] we can then calculate the exact position at which

[1] Chauncy (Sir Henry), *The Historical Antiquities of Hertfordshire* (London, 1700), I, p. 88 (reprinted in 1826). Quoted by Nissen, *Orient*, p. 7.

[2] Quoted by Nissen, *op. cit.*, pp. 7–8.

[3] *I.e.* from 24° 7′ to 23° 26′ between 4000 B.C. and A.D. 2000, according to Newcomb's calculations; *cf.* Table II (p. 112).

[4] *I.e.* about 25,800 years for a complete rotation.

a given star or the sun would rise upon the horizon, with due allowances for obstacles, for refraction, and for parallax, at any given day in remote antiquity. And we can compare these with the present-day orientation of the building, in an attempt to determine how much time has elapsed since its erection. But at the conclusion of this process, what have we attained? In many cases, to be sure, we have a fairly approximate or even conclusive demonstration that the axis was laid down with reference to astronomical conditions, in general accord with the evidence deduced from literature. With regard to determination of the date, however, precession and obliquity vary so slightly that estimates based solely on observation of the sun are valid only within a bracket of several centuries,[1] and hence of no value whatever when, as in the cases of most Greek temples, we already know the date within a single century on other grounds. It is not difficult to understand, therefore, why efforts should have been made to attain greater exactness by introducing additional astronomical factors. But these additional factors invariably rest on baseless hypotheses. One such hypothesis is that not only the original process of laying down the axis, but also each recurring anniversary, was duly celebrated at the temple, and that the priests, in the absence of night-working clocks, relied upon some designated star, rising or setting on the same axis, which would inform them that sunrise was due within one and a half or two hours. Then, finding that no such heliacal rising or setting of a star on the axis occurs in our own time, the nearest suitable star is arbitrarily selected for the purpose of computing the date at which it would have fulfilled the conditions. The result is the date of the building of the temples. To illustrate the workings of this hypothesis, let us glance at the dates obtained by Penrose for the Greek temples to be subsequently considered:

[1] Thus Admiral Somerville (*Antiquity*, I, 1927, p. 38) states that "even if we accept (as many do not) that there is in any prehistoric structure an intentional orientation to a rising or setting body, it may confidently be said that it is not possible to ascertain the date of erection of any such monument through a *solar* orientation."

TABLE I

DIVERGENCE OF ASTRONOMICAL AND ARCHÆOLOGICAL EVIDENCE FOR GREEK TEMPLE DATES

Name of Temple		Penrose's Astronomical Date	Archæological Date of Beginning	
			Visible Temple	Earlier Fdns
Parthenon,	Acropolis of Athens	1495 B.C., April 29	447 B.C.	c. 490 B.C.
Peisistratid temple,	Acropolis	2020 B.C., April 20	c. 535–525 B.C.	?
Erechtheum,	Acropolis . . .	447 B.C., September 2	c. 421 B.C.	c. 479 B.C.
Nike temple,	Acropolis .	1130 B.C., March 17	c. 427 B.C.	c. 479 B.C.
Olympieum,	Athens . . .	174 B.C., March 27	174 B.C.	c. 525–515 B.C.
"Theseum,"	Athens	466 B.C., October 6	c. 450–440 B.C.	?
Old Nemesis temple,	Rhamnus	1092 B.C., September 17	c. 530–490 B.C.	?
New Nemesis temple,	Rhamnus	747 B.C., September 22	c. 438–421 B.C.	—
Heraeum,	Olympia . .	1445 B.C., September 12	c. 610–600 B.C.	c. 700 B.C.
Zeus temple,	Olympia	790 B.C., April 6	c. 472–460 B.C.	—

With the exception of the Heraeum at Olympia, the archæological dates of all these temples lie between 535 and 420 B.C., a bracket of 115 years rather than 1846 years.[1] In this table, archæology and astronomy diverge by as much as 1490 years.[2]

Such flagrant discrepancies necessarily called for attempts at reconciliation. The explanation hitherto offered is that, if we are inclined to accept the archæological evidence for an existing temple, it must be regarded as lying on older foundations which fixed the orientation at some much more remote period. It is true that earlier foundations exist under the Parthenon, the Peisistratid temple, the Erechtheum, the Nike temple, the Olympieum at Athens, and the Heraeum at Olympia; but no such foundations, unless they be Mycenæan, can run back into the second millennium B.C.; the classical Greeks themselves did not think of erecting temples before 900 B.C. or later, and in at least five of these six instances the earlier foundations are plainly Greek, of about 490, 479, 520, and 700 B.C. We should have to assume that under these

[1] The Olympieum at Athens, formerly thought to have been laid out in 174 B.C., is now known to rest on foundations of which the orientation was determined about 525–515 B.C. Consequently the latest archæological date in the table is really not 174, but about 420 B.C.

[2] The reason for the comparative accuracy of three astronomical dates given in this table (Erechtheum, Olympieum, and "Theseum") is that they were based by Penrose primarily on archæological rather than astronomical evidence.

earlier foundations, in turn, were others still older, such as exist in fact under the Olympieum, but here following a different orientation and so not responsible for the present axis.[1] Under the Nike temple, furthermore, the earlier foundations lie at quite a different angle from the present temple;[2] consequently the present orientation was determined for the present temple, at about 427 B.C. rather than seven centuries earlier. Similarly the orientations of the new temple of Nemesis at Rhamnus and of the temple of Zeus at Olympia were clearly determined expressly for the present temples, begun respectively at about 438–421 and 472–460 B.C. For both are merely replacements, at divergent angles and at some distance from their predecessors, not resting on older foundations; and excavation below the temple of Zeus at Olympia has shown that it rests on an artificial plateau which buried statue pedestals of about 480 B.C. The exaggerated astronomical dates, therefore, may all be proved to be impossible. Is the orientation theory a fallacy?

If we are to undertake an impartial investigation of the orientation theory, we must first strip away the improbable assumptions accompanying it. No ancient people possessed such a calendar as would bring any designated moment in the same relationship to sun or stars in recurring years. Uniform deficiencies of one quarter day in Egypt, spasmodic deficiencies or augmentations of ten, eleven, or nineteen days in Greece, made each year differ astronomically from its neighbors. Repetition of the observations each year was both needless and meaningless; and the priests had no need, for this purpose, of astronomical clocks or star alarms.

For the purpose of investigation, we may select a few monuments concerning which we have adequate data, in order to ascertain how far archæology and astronomy can wander hand in hand.

[1] The older foundations under the Olympieum, differing by 2°, were assigned by Penrose to 1000 B.C. (letter in Waldstein, *Argive Heraeum*, I, p. 29).

[2] Stevens (*Hesperia*, 1936, p. 446) gives the difference as about 20°, but this seems slightly exaggerated.

Equinoctial and Solstitial Orientation
Giza and Stonehenge

As an early and most notorious example we may take the great pyramid of Khufu at Giza—notorious because of the undue mantic or oracular properties attributed to it by those who have been impolitely dubbed the "Pyramidiots." [1] This pyramid is laid out with such accuracy that it departs only 3 minutes of arc from true east, the true equinoctial point of sunrise (Fig. 1); and the second and third pyramids deviate

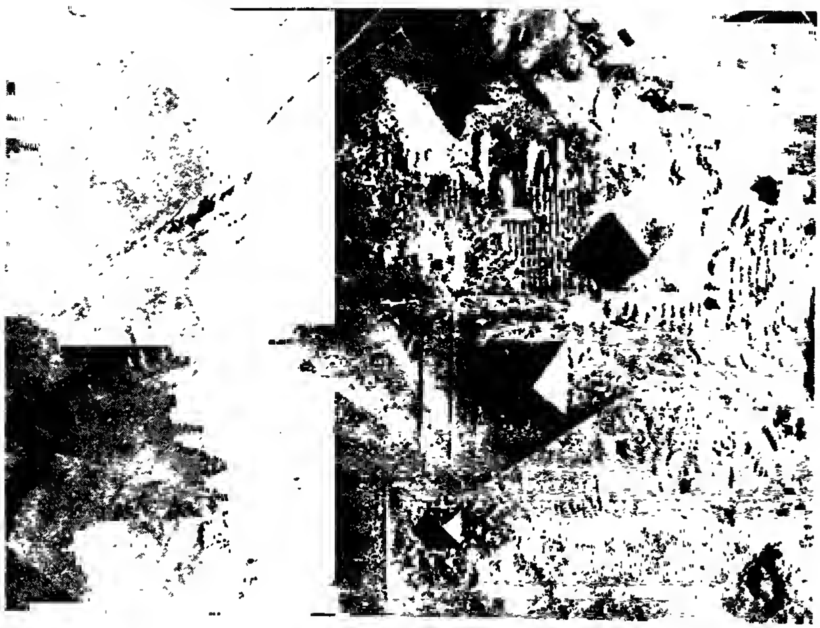

Fig. 1. Pyramids at Giza.

only 5½ and 14 minutes respectively from true east, implying fairly accurate observation for monuments erected five thousand years ago.[2] Recall that the greatest Greek astronomers

[1] Concerning these fantastic theories, which do not here concern us, see Borchardt, *Gegen die Zahlenmystik an der grossen Pyramide bei Gise* (Berlin, 1922); Wheeler, "Pyramids and their Purpose· Pyramid Mysticism and Mystification," in *Antiquity*, IX, 1935, pp. 292–304.

[2] The three orientations at Gizeh are, first, 0° 3′ 43″ north of east; second, 0° 5′ 26″ north of east; third, 0° 14′ 3″ south of east (see Petrie, *Pyramids and Temples of Gizeh*, pp. 39, 97, 111). The first pyramid was more accurately sur-

midway in date between the fourth Egyptian dynasty and ourselves made errors of at least one day, equivalent to 31 minutes of arc (the entire visual diameter of the sun), in computing dates of the equinoxes.[1] It seems obvious that the pyramid builders were striving to place their axes as nearly east-and-west as the mechanical means at that time would permit. Presumably their method was one of empirical observation rather than of calculation; the process of stretching lines on a level desert from a stake in the directions of sunrise and sunet, on any day of the year (but preferably toward the time of the solstices, when the declination changes most slowly), so that a line bisecting the angle thus formed yields a true north-and-south line,[2] and one at right angles a true east-and-west line. The slight discrepancies are exactly such as would result from the employment of such a method, of which the faults are the shifting relationships of sunrise and sunset to the equinoctial points.[3] But equinoctial orientation is of no assistance in determining the dates of the monuments; for these we have to rely solely on our archæological and historical evidence.

Turning to a later period and to a very different civilization, we examine the most notable of the megalithic monu-

veyed by Cole (*Determination of the Exact Size and Orientation of the Great Pyramid of Giza*, Survey of Egypt Paper No. 39, Cairo, 1925; *cf.* Borchardt, *Längen und Richtungen der vier Grundkanten der grossen Pyramide bei Gise*, Berlin, 1926), giving 0° 1′ 57″ and 0° 2′ 28″ north of east for the south and north sides respectively (averaging 0° 2′ 12″), and 0° 2′ 30″ and 0° 5′ 30″ west of north for the west and east sides respectively (averaging 0° 4′ 0″); general average 0° 3′ 6″.

[1] Ptolemy of Alexandria calculated a summer solstice at 2 A.M. on June 25, A.D. 140, which was about 36 hours too late (see my *Archons of Athens*, p. 317). This calculation, using the distances to equinoctial points (94½ and 92½ days before and after the summer solstice) which Ptolemy had inherited from Hipparchus, placed the equinoxes at 2 P.M. on the afternoons of March 22 and September 25, whereas the true equinoctial dates were then March 21 (7 P.M.) and September 24 (6 A.M.). Meton of Athens calculated the summer solstice as the early morning of June 27, 432 B.C., which was about 32 hours too soon; but we have not exact information as to the locations of his equinoctial points.

[2] The north-and-south axis was evidently the one taken as a working basis, for it is accurately engraved under the north side of the great pyramid at Giza (Cole, *op. cit.*, p. 8; Borchardt, *op. cit.*, pp. 3, 8). A similar north-and-south axis is engraved under one of the pyramids at Abusir.

[3] The differences are too slight to have any connection with the shifting Egyptian year of 365 days, which caused the calendar to shift about 16 days in the reign of Khufu alone.

ments of western Europe. At Stonehenge in England (Fig. 2) the inner system forms a horseshoe plan opening toward the northeast; the axis of this horseshoe passes through unobstructed and unusually wide openings in the outer circles toward the northeast;[1] and the prolongation of the axis toward the northeast almost coincides with the axis of a

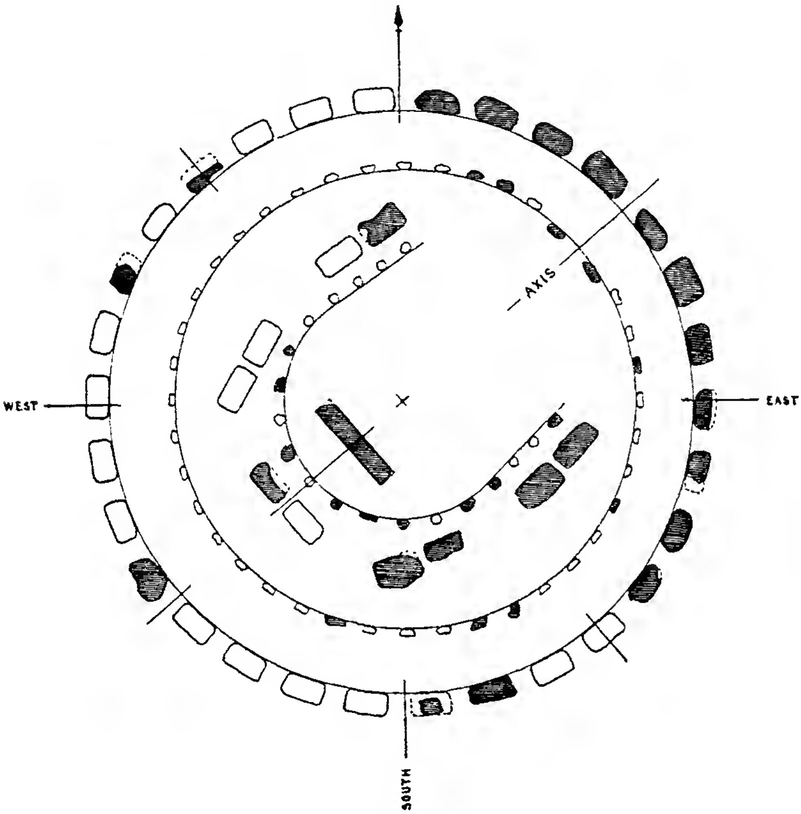

FIG. 2. Stonehenge.

great avenue 47 feet wide running northeast between parallel dykes for one third of a mile before it subdivides into two branches. Carried backward, the axis passed through the slit between the piers of the central trilithon (of which the

[1] The pair of outer circle stones at the northeast is 50 inches apart, that is, 12 inches farther apart than usual, definitely marking the entrance, and their inner edges are more carefully trimmed than the others.

southern pier has fallen so that the interval cannot be accurately measured),[1] and also through unobstructed intervals in the outer circles toward the southwest (though the southern pier of the outer pair again has fallen). But the axis could not have been sighted by an observer standing directly behind the central trilithon, since his view would have been blocked by the now missing central sarsen stone of the innermost row; in order to look over this, he must have receded to the surrounding embankment. This external position would have been most inappropriate, and would also have cut off the view of the two central piers at the northeast through which the observer should have sighted, these being hidden by the trilithon piers. Consequently the observer must have stood inside the innermost row, presumably in a marked position on the "altar stone" at the point where the axis crossed it.[2] Such are the indisputable general facts; the difficulty occurs when more detailed measurements are required for mathematical computation. The rough workmanship of the stones, and the fact that some of the most important stones have fallen, lead to certain discrepancies of observation. The most accurate observations of the bearing of the axis north of east are the five following:

Petrie (1880)[3]	39° 50′ 3″	(derived from stone circle)
Lockyer (1901)[4]	40° 24′ 9″	(derived from avenue)
Lockyer (1901)[5]	40° 25′ 42″	(derived from modern bench-mark on Sidbury Hill)
Stone (1923)[6]	40° 21′ 34″	(derived from avenue)
Cunnington (1935)[7]	40° 6′	(derived from stone circle)

[1] The central interval was estimated as 13 inches by Petrie, 22 inches by Lockyer, 30 inches by Stone (references as below).

[2] *Cf.* Cunnington, *Stonehenge*, pp. 63–66.

[3] Petrie, *Stonehenge—Plan, Description, and Theories* (1880); *cf.* Stone, *Stones of Stonehenge*, p. 134, for relation to later surveys. The axis is midway between the two outer circle stones at the northeast, but 20 inches from the northern pier of the axial pair at the southwest (the southern pier having fallen), and 7 inches from the northern pier of the central trilithon. The actual orientation (not given by Petrie) is calculated on the basis of figures quoted by Stone, to show that Petrie's axis diverges from his by 11 inches in 100 ft or 0° 31′ 31″ more to the southeast, and so 0° 35′ 39″ from Lockyer's preferred axis.

[4] Lockyer and Penrose, "An Attempt to Ascertain the Date of the Original Construction of Stonehenge from its Orientation," *Proc. Roy. Soc.*, LXIX, 1901,

Of these, the three observations by Lockyer and Stone are seemingly the most accurate, and show an almost negligible variation of 0° 4′ 8″, the average of the three being 40° 23′ 48″ north of east. But in the latitude of Stonehenge, 51° 10′ 42″, the theoretical northernmost point at which sunrise can appear above the horizon at the present day (maximum declination = obliquity of the ecliptic = 23° 26¾′) is that given by the following formula (wherein α is amplitude, δ is declination, and ϕ is latitude):

$$(1) \qquad \sin \alpha = \frac{\sin \delta}{\cos \phi} = \frac{0.3978}{0.6269} = 0.6346,$$
$$\alpha = 39^\circ\, 23\tfrac{1}{2}'.$$

This is the amplitude, north of east, at which the center of the sun's disk would rise above the theoretical horizon on the summer solstice. We must allow, however, for the apparent altitude of the opposite horizon, which is 0° 35′ 48″.[8] Also, the sun casts sufficient illumination and is sufficiently visible to permit the observation of its axis (or bisection of its cap or upper limb) when the top is only one-eighth of the radius

pp. 137–147; Lockyer, *Stonehenge, Astronomically Considered* (1906). The axis is midway between the two piers at the northeast, but 32½ inches from the northern pier of the southwest pair, and 11 inches from the northern pier of the central trilithon. The actual orientation was derived from the mean between two slightly divergent bearings of two different portions of the avenue.

[5] This modern bench-mark was adopted by Lockyer because of the close coincidence in the bearing, on the theory that the ancient builders may similarly have chosen this point for an observation mark.

[6] Stone, *Stones of Stonehenge* (1924), pp. 20–30, 125–127, 129–134. The axis is still midway between the two piers at the northeast, but 31 inches from the northern pier of the southwest pair, and 18 inches from the northern pier of the central trilithon. Stone does not mention the actual orientation, but from his figures (*l.c.*, p. 133, Pl. 35) which show that he deviates 3 inches in 208½ feet (outer circle to "heel stone") or 0° 4′ 8″ more to the southeast than Lockyer's preferred axis, the result is determined.

[7] Cunnington, *Stonehenge and its Date* (1935), pp. 51–70. Cunnington does not include fresh observations, but, noting that Lockyer's axis is discrepant by 3½ inches in the radius of the outer circle (50½ feet), he estimates that the discrepancy of 3½ inches in 50 feet is 0° 20′, the amount by which Lockyer's amplitude must be reduced.

[8] The altitude is that of Lockyer (*Proc. Roy. Soc.*, 1901, p. 146); in his later work, as quoted also by Stone (*op. cit.*, p. 27), he preferred 0° 35′ 30″, a difference of no effect in this calculation.

or 0° 2′ above the apparent horizon,[1] so that the center would be apparently 0° 14′ below; and at the altitude of the apparent horizon the refraction (including parallax) would be 0° 28′ 35″,[2] so that the true center would be 0° 42′ 35″ below the apparent horizon, or 0° 7′ below the theoretical horizon.[3] We apply the following differential formula (in which d is the difference between true and apparent in each case):

$$(2) \quad d\alpha = \frac{\tan\phi}{\cos\alpha}\,dh = \frac{1.2428}{0.7728} \times 7' = 1.6082 \times 7' = 11\tfrac{1}{4}'.$$

Thus the observer to-day would see the sunrise at 39° 23½′ + 0° 11¼′ = 39° 34¾′ north of east,[4] which is 0° 49′ farther south than the average result for the three seemingly most accurate observations of the orientation of Stonehenge.

The difference may be attributed to the variation in the obliquity of the ecliptic, which has to be taken into account in dealing with all ancient monuments which are not oriented exactly according to the equinoxes. This obliquity (equivalent to the maximum declination of the sun or distance of the

[1] The estimate of 0° 2′ is that utilized by Lockyer and Penrose at Stonehenge, and agrees with my own experience at Athens. The amount, to be sure, has been disputed. Somerville, in the passage quoted above, continues, "we do not know—and probably never shall know—what *particular moment* of the phenomenon of sunrise was chosen by the builders for the laying out of the desired line on the ground that was to be the orientation of the building . . . between the moment when the 'first flash' of the sun's upper rim is sighted, and the moment when the whole sun is seen standing (as it were) on the horizon. . . . 'First flash' is just as likely a moment as 'whole orb.' We do not know *which* to employ, and this it is which makes dating by azimuth of sunrise, whether at Stonehenge or anywhere else, impossible." There can be no reasonable doubt, however, that the "first flash" was the striking and psychological moment. This was adopted by Petrie, Stone, Cunnington, and other observers, and seems unquestionable. The only doubt attaches to the precise amount of the sun's disk which can be assumed to have appeared. Lockyer calculated that the allowance of 0° 2′ would cause a difference of 17 seconds of time, or 0° 3′ 15″ of amplitude, as compared with the assumption that the top of the sun's disk coincided with the horizon.

[2] See table (IX) of refractions on p. 149.

[3] Lockyer's calculation was slightly different: horizon 0° 35′ 48″, increased by apparent altitude of upper limb to 0° 37′ 48″, diminished by refraction and parallax (27′ 18″) to 0° 10′ 30″, diminished by the sun's radius (15′ 46″) to 0° 5′ 16″ below the theoretical horizon.

[4] By actual observation, Lockyer obtained 39° 33′ 30″ north of east, closely coinciding with the result obtained from the formula, though it must be remembered that his observation was taken four days late (June 25, 1901).

sun's center from the celestial equator) is now $23° 26\frac{3}{4}'$ and is constantly diminishing. Receding to antiquity, we find that the obliquity was larger in accordance with the following tabulation:[1]

TABLE II

Variation of the Obliquity of the Ecliptic

2500 B.C.	23° 58′ 44.00″	A.D. 0	23° 41′ 43.78″
2000	23° 55′ 38.99″	500	23° 37′ 57.97″
1500	23° 52′ 23.10″	1000	23° 34′ 8.07″
1000	23° 48′ 57.70″	1500	23° 30′ 15.43″
500	23° 45′ 24.14″	2000	23° 26′ 21.41″
0	23° 41′ 43.78″	2500	23° 22′ 27.37″

The date at which Stonehenge was laid out has been widely discussed; and even recently the theories have varied from 2000 B.C. to A.D. 750. If we have sufficient faith in the accuracy of the orientation $40° 23\frac{3}{4}'$, however, we apply first the differential formula,

$$(2) \quad d\alpha = \frac{\tan \phi}{\cos \alpha} dh = \frac{1.2428}{0.7616} \times 7' = 1.6318 \times 7' = 11' \, 44'',$$

so that the theoretical amplitude of sunrise on the solstice would have been about $40° 23\frac{3}{4}' - 11\frac{3}{4}' = 40° 12'$ at the time of the erection of the monument. Next we apply the following formula,

$$(1) \quad \sin \delta = \sin \alpha \cdot \cos \phi = 0.6455 \times 0.6269 = 0.4047,$$
$$\delta = 23° 52\frac{1}{2}'.$$

This maximum declination (obliquity of the ecliptic) corresponds to a year about 1500 B.C.[2]

The basic difficulty with this calculation is that the three observations on which it is founded have no more than a specious accuracy. In order to secure a longer line than that afforded by the radius ($50\frac{1}{2}$ feet) of the monument itself, they have either employed the axis of the avenue, one third

[1] These are the results attained by Newcomb (*Fundamental Constants of Astronomy*, 1895; *Encycl. Britt.*, 11th ed., VIII, p. 895). Stone (*Stones of Stonehenge*, Pl. 8) plots the results in the form of a curve.

[2] By similar methods, from the results given in the tabulation, Petrie obtained A.D. 730, Lockyer 1680 B.C., Stone 1840 B.C., and Cunnington 400 B.C.

of a mile long [1]—which, however, runs down hill and so looks convincing only on a modern map—or have substituted a modern bench mark on Sidbury Hill eight miles away—which is, however, a modern surveying mark and may have had no ancient prototype, while the horizon on which it stands is invisible from Stonehenge so that the connection is imperceptible except on a modern map. The only safe procedure is to confine our attention to the monument itself. It has been pointed out that if we correct Lockyer's Sidbury Hill axis to the extent of its deviation of 3½ inches within the radius (50½ feet) of the outer circle alone, the orientation should be reduced by about 0° 20′,[2] perhaps more accurately 0° 19′ 52″, thus giving about 40° 5′ 50″. We must take into consideration also the "heel stone," with its peak located 256 ft from the center of the circle.[3] Of its purpose we have for the first time a reasonable interpretation in Petrie's recent suggestion that it was so placed as to intersect the horizon at the point where the sun rose fully clear,[4] combined with Cunnington's observation that when the observer stands on the "altar stone" the peak of the "heel stone" does not hide the horizon but coincides with it.[5] The distance from the rear edge of the "altar stone" to the center of the circle being 12 ft, or rather 11 ft for an observer standing on it, the length of the line would be 267 ft. The peak of the "heel stone" was 72 inches to the right of Lockyer's axis, or 69 inches to the right of Stone's axis; such deviations, in the distance of 267 ft, would be equivalent to 1° 17′ 13″ or 1° 14′ 1″ respectively, giving 39° 8′ 29″ or 39° 7′ 33″, mean 39° 8′. Petrie had observed in 1880 that the sun passed through 0° 53′ of amplitude in rising clear of the horizon, of which amount we may assume that 0° 3′ were consumed by the rising of the cap to the

[1] The Lockyer survey of the avenue, furthermore, consisted in obtaining the directions of two different sections of the avenue and adopting a mean result, a further source of doubt.

[2] Cunnington, *op. cit.*, pp. 67–68.

[3] See drawings by Stone, *op. cit.*, Pl. 34, 35.

[4] Petrie, *Man*, 1924, p. 107.

[5] Cunnington, *op. cit.*, pp. 63–69.

observation point.[1] Hence, if Petrie's suggestion regarding the "heel stone" be correct, we have only to add 0° 50′ to the amplitude of the "heel stone" as seen from the "altar stone," giving 39° 58′ as the amplitude of sunrise at the time of erection. This coincides so closely with the corrected axis of the circle (40° 6′) as to form satisfactory corroboration. Accepting, therefore, the orientation as about 40° 6′, indicating that the solstice sunrise at the time of erection was about 0° 31¼′ farther north than at present,[2] we apply first the differential formula,

$$(2)\quad d\alpha = \frac{\tan\phi}{\cos\alpha}dh = \frac{1.2428}{0.7649}\times 7' = 1.6248\times 7' = 11'\,22'',$$

so that the theoretical amplitude of sunrise on the solstice would have been about 40° 6′ − 11½′ = 39° 54½′ at the time of the erection of the monument. Next we apply the formula

$$(1)\quad \sin\delta = \sin\alpha\cdot\cos\phi = 0.6416\times 0.6269 = 0.4022,$$
$$\delta = 23^\circ\,43'.$$

This maximum declination (obliquity of the ecliptic) corresponds to a year about 175 B.C. But considering the roughness of the work, in which a deviation of 1 inch in the line from the "altar stone" to the outer circle (61½ ft) changes the amplitude by 0° 4½′, equivalent to 330 years, it is apparent that we must allow a leeway of several centuries, and preferably before rather than after. It is highly improbable, however, that we should allow so great an error as to carry the date back into the second millennium. A date in the first millennium B.C., furthermore, is in closer agreement with the results of recent excavations.

In any case, the fact is demonstrable that at Stonehenge we are dealing with solstitial orientation. Only the roughness of the workmanship, combined with the slow variation in obliquity, prevents us from obtaining a more precise date.

[1] See p. 114, n. 1.

[2] Interesting corroboration of this result is the fact that Petrie, by direct observation in 1880, from a position only slightly behind the "altar stone" (behind the trilithon), observed the interval between the peak of the "heel stone" and the center of the sun when clear of the horizon as 1° 55′–1° 24′ = 0° 31′.

Orientation throughout the Solstitial Arc

Greece

Turning next to the temples of ancient Greece, where we have ascertained that there existed a certain amount of literary tradition with regard to orientation toward the rising or setting sun, it is to be observed that the actual examples box the entire compass, with no special predilection for equinoctial or solstitial orientation.[1] If, nevertheless, we compile a list of Greek temples with their orientations, or a chart illustrating the various angles in graphic form (Fig. 3), such as the list of one hundred and ten published by Nissen,[2] it is remarkable that 73 per cent fall within an arc of 60 degrees facing east, leaving only 27 per cent for the remaining five-sixths of the circumference. Nearly a third of this residue, furthermore, or 8 per cent of the total, fall within another arc of 60 degrees facing west,[3] leaving only 19 per cent for the remaining two-thirds of the circumference. And while it is true that this list includes a mere fraction of the total number of temples now known, yet it is improbable that the proportions would be appreciably changed by lengthening the list.[4] In seeking the meaning of this preponderating concentration of 81 per cent within one-third of the circumference, or of 73 per cent within one-sixth of the circumference, we examine the arcs within which sunrise and sunset occur at this latitude. The amplitude of sunrise at the solstices may be found by

[1] With axes running in every direction, it is natural that some should be exactly, or almost exactly, east-and-west, toward the equinoctial sunrise or sunset. This is true of temples at Acragas, Magnesia, and Priene, as well as of the temple of Zeus Olympius at Athens. But it would be illogical to single these out as instances of the application of a special theory; and in the case of the temple of Zeus Olympius, at least, the evidence points to an occasion six days from the equinox.

[2] Nissen, *Orient.*, pp. 244–247, nos. 1–113 (omitting nos. 4, Ancona; 77, Artemis Brauronia; 87, Pnyx).

[3] *I.e.* nos. 7–88 (80 examples) toward the east and nos. 100–108 (9 examples) toward the west.

[4] In any case, so little attention has been paid in the last thirty years to exact astronomical observations of orientation, that it would be difficult to lengthen the list by means of existing publications. A supplementary series of original surveys would be required.

the following formula, wherein α is the amplitude, δ the declination (at the solstice dates identical with the obliquity of the ecliptic, now 23° $26\frac{3}{4}'$), and ϕ the latitude (37° 58′ 20″

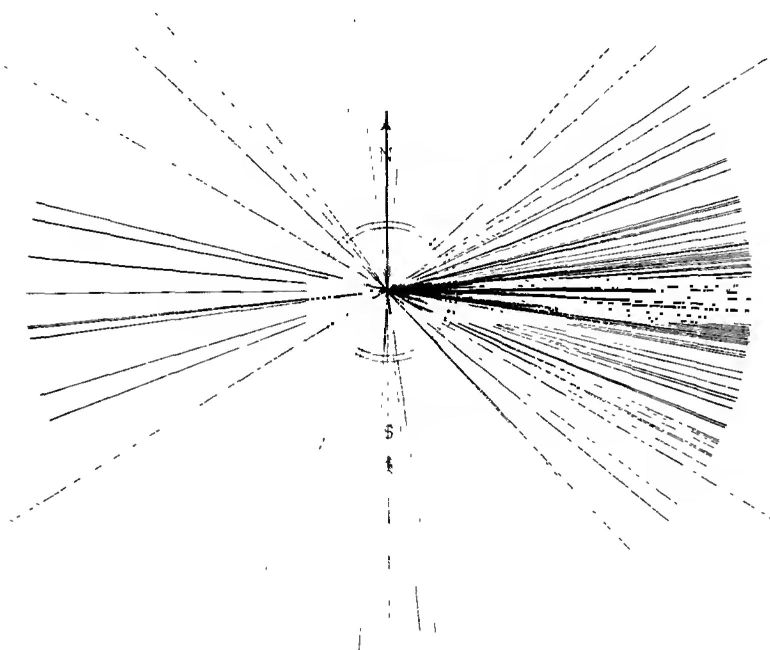

Fig. 3. Frequency chart of Greek temple orientations.

at Athens):

$$(1) \qquad \sin\alpha = \frac{\sin\delta}{\cos\phi} = \frac{0.3978}{0.7883} = 0.5046,$$

$$\alpha = 30° \; 18\tfrac{1}{2}'.$$

Hence the theoretical solstitial arc, the arc enclosed between the true sunrise or sunset points at the summer and winter solstices, now amounts to 60° 37′. Transferring ourselves back to antiquity, we have to allow for the fact that the obliquity of the ecliptic at the central date 500 B.C. was about

$23° 45\frac{1}{2}'$. Again using the formula,

$$(1) \qquad \sin \alpha = \frac{\sin \delta}{\cos \phi} = \frac{0.4028}{0.7883} = 0.5110,$$
$$\alpha = 30° 44',$$

we see that the theoretical solstitial arc would then have been 61° 28′. It is unnecessary to make the small allowances for obstacles and for refraction, differing in every instance. It is sufficiently obvious that the solstitial arc determined the normal range of variation. The 19 per cent of the total number of temples falling outside this range must have been regulated by special considerations, many of them of local character,[1] which would have to be separately investigated for each individual case.[2]

Another significant fact is usually observed when two Greek temples are built side by side, or one over the other. Under such circumstances it would seem to us most natural, especially when both were dedicated to the same worship and one was to supplant the other, to make them parallel, or to superpose one directly on the other's foundations. With the Greeks, however, such logical procedure was exceptional. Very rarely were adjacent temples made parallel; those of Apollo at Delos are among the exceptions. More frequently we find parallelism when the temples are directly superposed; but here the explanation is usually the economy of using the older foundations, or particular respect for the old tradition. Such instances are the present Parthenon on the Older Parthenon substructure, and the Seleucid Olympieum utilizing the Peisistratid platform, similar reasons dictating the emplacement of the temples of Poseidon at Sunium, of Apollo at

[1] That such exceptions must occur is specifically stated by Vitruvius.

[2] Thus in one of the exceptions, the temple of Apollo at Bassae (which runs almost exactly north-and-south), the determining factor was undoubtedly tradition, following approximately the direction of the older and more primitive temple, which in turn was apparently located in accordance with the site, a ridge running north-and-south offering a natural foundation which at such an early date would have been adopted in preference to the expedient of erecting a heavy foundation lying transversely across the ridge.

Delphi, and of Artemis at Magnesia; religious conservatism was more probably responsible for the Erechtheum enveloping an earlier shrine, and for the Heraeum at Olympia and the temples of Artemis at Ephesus in superposed strata above their predecessors. By far the majority of cases show divergent angles, whether the older temples lie to one side or directly below. Thus the Parthenon and the Erechtheum on the Acropolis converge eastward at 8° 2′ (Fig. 7), and the older foundation between them nearly, but not quite, bisects the angle. The two temples at Rhamnus converge at 2° 55′, the two at the Argive Heraeum by 1° 14′, the two at Olympia by 3° 36′, while the two at Samothrace disagree by 28°. Even when temples are directly superposed we find that the axes are frequently changed; examples are the temple of Athena Nike on the Acropolis, the Peisistratid Olympieum at an angle to the older foundations below, the marked difference between the successive temples of Artemis Orthia at Sparta, the angle of 12° 40′ between the axes of the superposed temples at Locri Epizephyrii in South Italy. It is not easy to find an explanation for such a general law, except an astronomical one. But the pairs of temples sometimes converge eastward, sometimes diverge; the later axis sometimes points more to the north than its predecessor, sometimes more to the south. If these be the effects of astronomy, it is not the organized movement of the heavenly bodies, but an astronomy into the machinery of which man has thrown a monkey-wrench—his calendar; for the Greek calendar was always a lunar one, and its erratic course with respect to the sun would naturally result in irregularities of exactly this sort. And if the temples can be associated with the festivals of the gods, and these in turn with the lunar calendar, and if the variations of the lunar calendar with respect to the sun can be ascertained, it would seem that we might make considerable progress toward understanding the Greek attitude with regard to orientation.

It is obvious that, in order to verify the relationship between temple axes and astronomical observations, we cannot restrict the investigation to natural phenomena, such as the

equinoctial sunrise in connection with the Egyptian pyramids, or the solstitial sunrise at Stonehenge. Under no circumstances could we apply the law of the variation in the obliquity of the ecliptic, which in any case, with its difference of merely 0° 0′ 37″ per century at that time, would be too infinitesimal to aid in dating temples already located within twenty or thirty years on other grounds. And we have learned to be wary of the precession of the equinoxes and the shifting positions of warning stars.

Perhaps we may approach the problem by devising a pseudo-algebraic equation somewhat in the following form,

$$X = Ar + R + C + As,$$

wherein X represents the usually unknown date, Ar the archæological evidence as to the date, R the religious evidence as to the cult, C the artificial astronomy of the local calendar, and As the natural astronomical observations. If only one of these elements be unknown, the others may, perhaps, supply it; but if more than one or two be unknown, the solution will probably be found to be hopeless.

The Example of the Parthenon

Late on the evening of last August 22 (1937), a Sunday night, a few of us were sitting on the terrace of the American School at Athens, gazing at the stars and the full moon, when someone asked if the morrow were not an important occasion in my career. I was startled to recall that I had forgotten my own suggestion, published two years previously,[1] that somebody ought to observe the dawn of Athena's birthday from within the Parthenon. It was too late to secure a theodolite or to make elaborate preparation; but a dash to the Acropolis with the tools at hand, measuring tape, plumb line, and camera, was the only method of redeeming the situation. Here, then, I awaited the calculated moment, under conditions very different from those of antiquity; for now the Par-

[1] *Amer. Journ. Archæology,* 1935, p. 509, n. 1.

thenon was illuminated by the full moon, whereas on the night of Athena's birthday the moon was never visible.[1]

I had selected the Parthenon as a test case because of the facility with which so many of the elements of our algebraic equation could be determined. It is true that earlier calculations had not yielded results such as to inspire confidence: Penrose, as noted before, had obtained 1495 B.C., while Burnouf calculated 554 B.C., and Mommsen 458 B.C.[2] It is true, furthermore, that we know the actual date of the Parthenon which we see to-day, begun in 447 B.C. But it has been recognized ever since excavations in 1836 that the present Parthenon rests on, and that its direction was regulated by, an older foundation, a massive platform erected for an earlier temple of somewhat different proportions. It is the date of this earlier platform which we must seek. Without going into details as to the controversy over its date,[3] it is very clear that the archæological study of the stratification limits us to a very narrow bracket after 490 B.C., the year of the battle of Marathon, and before 480 B.C., when the Acropolis was devastated and burnt by the Persians—and, furthermore, to a year very early within this bracket, since the foundations were erected to a height attaining 34½ feet (10.50 m) and surmounted by the three-stepped platform and by the lowest drums of the marble columns before the catastrophe. The religious evidence suggests that the lines of the foundation

[1] That is, Athena's birthday being the third day from the last of a lunar month (Hekatombaion), and the first "business day" of the next month being the day after the crescent of the new moon (the "new light") was first visible, it is clear that her birthday fell in the period of darkness between the old and new moons. This was the case, at least, before Meton's reform of the calendar in 432 B.C.; but he seems to have begun the month a little earlier, estimating the true moment of the conjunction (Dinsmoor, *Archons of Athens*, pp. 313–317), so that the disappearing old crescent might sometimes have been visible on the night before Athena's birthday.

[2] Penrose, *T. R. S.*, CLXXXIV, 1893, pp. 809, 827, 828; CXC, 1897, p. 45; Burnouf, *Legende Athénienne*, pp. 53–61; *Ville et Acropole d'Athènes*, p. 179; A. Mommsen, *Bursian's Jahresbericht*, LXXIII, 1892, pp. 22–25; *Feste der Stadt Athen*, p. 55.

[3] See my own articles, *A. J. A.*, 1934, pp. 408–448; 1935, pp. 508–509; *Jahrbuch deut. arch. Inst.*, 1937, pp. 3–13.

were laid out on Athena's birthday, Hekatombaion 27 (28),[1] at sunrise, the moment when the Panathenaic procession began to move from the lower city[2]—though it is unnecessary to assume that this was necessarily a celebration of the quadrennial Great Panathenaia, since it would have been unreasonable to have deferred such a project three or four years. As for the Athenian calendar, it must be admitted that for this early period we have not many indications; but a solar eclipse of October 2, 480 B.C., at the end of the third month of the Athenian year, fixes the preceding new year as July 7, and so Hekatombaion 27/28 as August 2/3. Receding from this fixed point, the corresponding equivalents of Hekatombaion 27/28 in the other years of the decade would be as follows: 481, July 15/16 or August 14/15; 482, July 27/28 or August 26/27; 483, July 8/9, August 7/8, or September 6/7; 484, July 18/19 or August 17/18; 485, July 30/31 or August 28/29; 486, July 11/12 or August 10/11; 487, July 22/23 or August 21/22; 488, August 2/3 or August 31/September 1; 489, July 14/15 or August 13/14; 490, July 26/27 or August 25/26.[3]

The remaining element of the equation is the astronomical observation. The orientation of the Parthenon and its basement had been carefully obtained by Penrose as 12° 53′ north of true east. The axis, prolonged at this angle, would pass the crest of Mt. Hymettus (Fig. 5) at a distance of nearly $5\frac{2}{3}$ miles (9075 m), and a height of 2000 feet (610 m) above sea level, about 1475 feet (450 m) above the Parthenon basement, the angle rising to the visible horizon then being 2° 50′.[4]

[1] The third day from the end would be Hek. 27 or 28, depending on whether there were twenty-nine or thirty days in the month, the number being yet unknown and presumably varying irregularly though with some semblance of alternation.

[2] *IG.*[2] (*Inscriptiones Græcæ*[2]), II, 334; Himerius, III, 16.

[3] The uncertainties of one month in each year are due to the fact that we do not yet know the quality of these lunar years, that is, whether they were ordinary (with twelve months and so 354 or 355 days) or intercalary (with thirteen months and so 384 days).

[4] Penrose gives the altitude as 2° 25′, but this has been readjusted for a calculated amplitude of + 15° 52′ 41″, declination + 13° 57′, in accordance with his theory of heliacal stars.

Assuming that the sun would have been sufficiently visible when one-eighth of its radius or 0° 2′ of its diameter rose above the mountain crest, and noting that the refraction at this angle (allowing for parallax) is 0° 15′, it is apparent that the true altitude of the sun's top at the moment of observation would have been 2° 37′, and hence the true altitude of the sun's center 2° 21′. At this point the sun would rise, as seen from the Parthenon, on two occasions in the year, sixty-three days before or after the summer solstice, and so at present on April 20 or August 23, but toward 500 B.C. on April 28 or August 31. The earlier date, in April, would be meaningless; but the later date, August 31, coincides exactly with one of the two allowable dates for Hekatombaion 27 in 488 B.C., and disagrees, furthermore, with either of the allowable dates in any other year of the decade. This coincidence is in agreement with the fact that 488 is exactly the year which seems most suitable, being sufficiently earlier than the destruction in 480 B.C. to give time for the erection of the unfinished temple, and yet at the same time the earliest possible occasion after the archonship of Aristeides (489/8) during which the plans, commemorating the Battle of Marathon, would most fittingly have been drawn up.[1]

Such were the calculations which it had been possible to work out at such a distance as New York. But, not being an astronomer, it was with considerable trepidation that I waited to see whether the sun would actually behave at Athens in accordance with the program. It was possible to select such a position, exactly on the measured axis at one end of the temple, that the visual angle enframed between the two central columns at the opposite end was 2° 4′, almost exactly four times the angular diameter of the sun at this period of the year (31¾′), and barely more than four times the daily variation of the axis of sunrise at this period (27′). Under such conditions the absence of a theodolite was not greatly to be regretted: the course of the sun could be followed with

[1] For a slightly different method of demonstration, but yielding the same result, see my article in *A. J. A.*, 1934, pp. 441–448.

sufficient accuracy by the angular measurements given by the columns. But as the pink glow heralding the dawn appeared low down on the slope of Mt. Hymettus, much too far to the left, something appeared to be wrong. The rising slope of the mountain, however, almost kept pace with the ascending path of the sun, until at last a blinding glare filled the central intercolumniation. And here, exactly at the center, the cap of the sun emerged (Fig. 4).

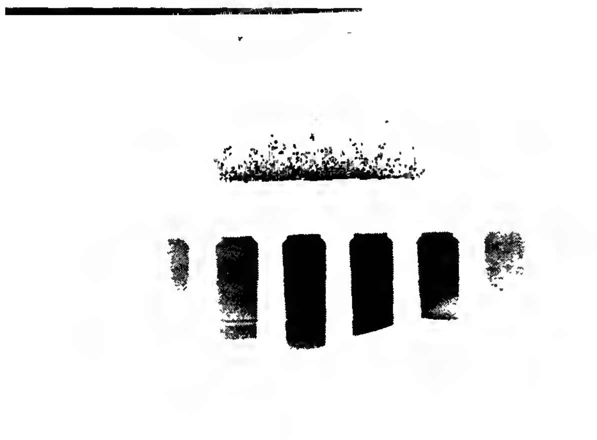

Fig. 4. Sunrise on the Parthenon axis, August 23.

Archæology

The Parthenon was logically the first example to be investigated, because the date of its foundation could be limited archæologically within such a narrow bracket, and all the other elements of the equation were likewise determinable, either exactly or with a fair degree of certainty. It now seems desirable to examine a few others of which the elements are less accurately known. We may select the three other temples on the Acropolis itself (the Peisistratid temple, the Erechtheum, and the temple of Athena Nike), two temples in the lower city (the Olympieum and the so-called "Theseum"), the two temples at Rhamnus (the old and new temples of

Nemesis), and two temples at Olympia (the Heraeum and the temple of Zeus).

We examine first the archæological evidence relating to these various temples, for the purpose of determining one of the unknown quantities (*Ar*) in our equation.

The archæological evidence demonstrates that the Peisistratid temple on the Acropolis dates from about 530–520 B.C., the date required by the architectural and sculptural style of the external colonnade; thus the temple should have been commenced in the early part of this decade. To include all possibilities, however, we may consider the entire quarter century 540–515 B.C. It has long been thought that this colonnade was merely wrapped around an older inner building of the first part of the century (which would, therefore, have determined the orientation); but I think it can be proved that the inner building is as late as the external colonnade.[1] In other words, we have no evidence that the axis of the Peisistratid temple was governed by an older temple; the somewhat nondescript foundations of a primitive structure at a lower level seem to be at a slightly divergent angle.

The Erechtheum and the temple of Athena Nike must be considered together. The latter is stylistically the predecessor of the other. Both must fit into the period between 437 B.C., when work was begun on the Propylæa, the older neighbor of the Nike temple, and 409 B.C., when the work on the unfinished Erechtheum was resumed. Allusions to statues dedicated in the precinct of Athena Nike in 425 and 424 would suggest that the temple was then in course of erection or already completed,[2] so that I assumed that its date was about 426 B.C.[3] This seems to be confirmed by two inscriptions, one known for forty years but only recently dated by Meritt as of 424/3 B.C., implying that the temple had then been com-

[1] This will be demonstrated in a separate study of the predecessors of the Parthenon.

[2] *IG.*², I, 368; II, 403.

[3] Dinsmoor, Anderson, Spiers, *Architecture of Ancient Greece* (3rd ed., 1927), pp. 126, 193. I had previously attempted to date the temple somewhat closer to 437, about 435–432 B.C. (*A. J. A.*, 1923, p. 320).

pleted,[1] and the other longer known but first attributed by West to 427/6 B.C., mentioning a temple, perhaps this temple, as about to be begun.[2] Thus we may assume that the temple was begun in 427 or 426 B.C. The Erechtheum (Fig. 7), begun after the Nike temple, presumably dates therefore from the period of the Peace of Nicias, 421–413 B.C., and the interruption of the work is probably to be attributed to the Athenian disaster at Syracuse.[3]

The Olympieum (Fig. 5), in its present form, was certainly the creation of Antiochus IV, begun in 174 B.C. It has now been ascertained, however, that the preceding temple which Vitruvius (VII, pr.) and Aristotle (*Pol.*, V, p. 1313 b) mention as an abortive creation of the Peisistratids is not represented by the older foundations at a slight angle to the present structure, as formerly thought, but rather by the lower steps of the present temple itself, clearly of late sixth century workmanship.[4] Because of the presence of the developed double-T clamps in these lowest courses, they cannot antedate the upper parts of the work in the Peisistratid temple on the Acropolis, about 525–520 B.C.; and they must be earlier than the expulsion of Hippias in 510, perhaps earlier than the assassination of Hipparchus in 514 B.C. But to include all possibilities we may consider the decades 530–510 B.C.

The so-called "Theseum," undoubtedly the temple of Hephæstus, must antedate the last years of work on the statues of Athena and Hephæstus (421–415), wherein the temple is mentioned as already existing and the doors are encased in timber for their protection.[5] It might be thought that the temple was begun in 421 B.C., perhaps simultaneously with the Erechtheum, when the festival of Hephæstus was

[1] *IG.*², I, 25; see Meritt and Davidson, *A. J. P.* (*Amer. Journ. Philology*), 1935, pp. 65–71.

[2] *IG.*², I, 111; see West, in Paton and Stevens, *Erechtheum*, pp. 647–648.

[3] An alternative theory, to which I formerly inclined (*A. J. A.*, 1932, p. 319), is that the Erechtheum was begun before the opening of the Peloponnesian War in 432 B.C. But this could not easily be reconciled with the evidence for the Nike temple.

[4] Welter, *Ath. Mitt.*, 1922, pp. 62–71.

[5] *IG.*², I, 371.

reorganized,[1] and when the statues are known to have been undertaken; and such a date would accord with the style of the sculptured inner friezes. But the style of the external

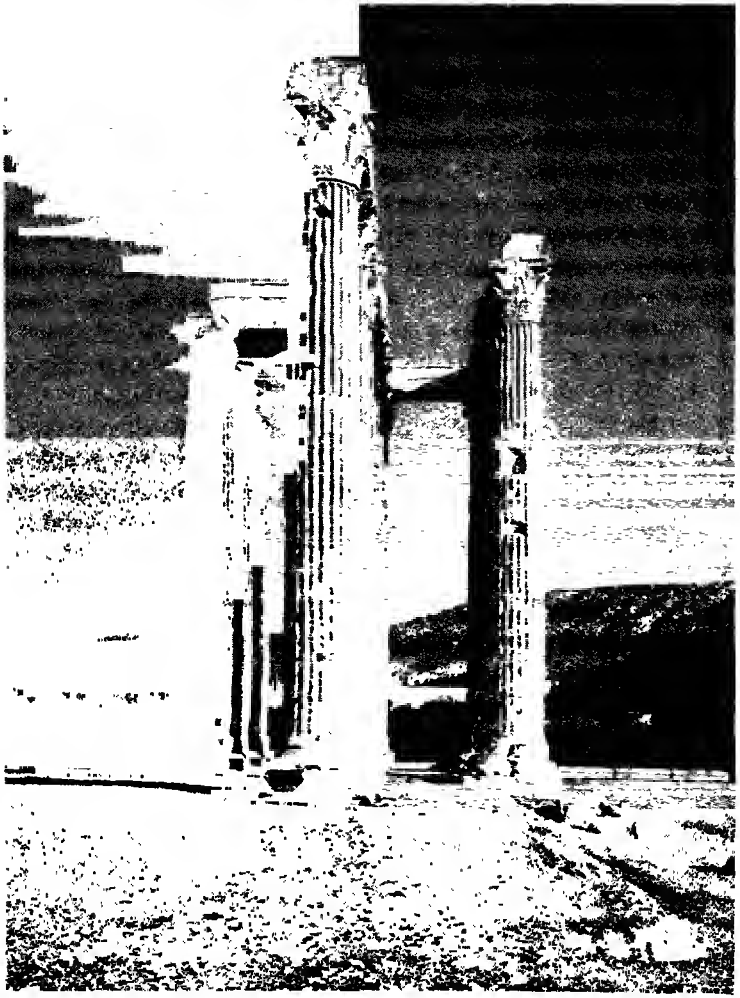

Fig. 5. Olympieum at Athens, with eastern horizon.

sculptured metopes seems to be about three decades earlier, suggesting that the exterior belongs to about 450 B.C.;[2] and

[1] *IG.*², I, 84.
[2] Koch, *Jahrb. deut. arch. Inst*, 1928, AA, 721.

such an early date is favored by the profiles of the moldings,[1] and also by the American excavations in the Agora, revealing that the pottery found among the marble chips and building debris of the temple comes down no later than the middle of the century.[2] To include all possibilities, we may consider the two decades 455–435 B.C.

Of the two temples of Nemesis at Rhamnus, one is clearly of the sixth century, the other of the fifth. The older is stylistically, judging from the architecture, of the last years of the sixth century; but we cannot decide whether it belongs to the last years of the tyranny (before 510), or to the first years of the democracy. For this reason we must leave a wide margin, about 530–490 B.C. The later temple is in many details a reflection of the Parthenon, of which the architecture was completed in 438 B.C.; and attribution to the period 438–432 B.C. would account for the fact that the columns are unfluted and the dressing of walls and steps incomplete, presumably because of the interruption caused by the Peloponnesian War. It would also be possible to assign the temple to a slightly later period, about 420 B.C., which might well be regarded as more suitable for the hesitant introduction of a new building material which did not become customary until the fourth century,[3] and for the sculptural style of certain fragments of relief from the pedestal of the cult statue; the known career of the sculptor of the statue, Agoracritus the pupil of Pheidias, could fit either period, and the incompletion of the temple could be attributed to the disaster at Syracuse as well as to the outbreak of the war. In short, we must scan every date within a quarter of a century, 438–413 B.C.

At Olympia, the existing Heræum must date back to about 600 B.C., but can hardly be much earlier, because of its

[1] Shoe, *Greek Mouldings*, passim.

[2] I am indebted to the kindness of Dr. Shear, and to the helpfulness of Miss Lucy Talcott, for an opportunity to examine the excavated material and to learn the opinions of the excavators.

[3] Breccia or conglomerate was employed for the first time, so far as we can ascertain, in the later temple at Rhamnus, and here merely in a subordinate capacity as the underpinning of marble floors, the builders hesitating to use it in foundations carrying the entire weight of the temple.

architectural style and because of the discovery of a small Corinthian vase under its external foundations.[1] The direction was fixed, however, at a much earlier period when the foundations subsequently utilized for the inner building were laid; and this earlier date cannot be defined more closely than the last quarter of the eighth century (725–700), as indicated by the style of votive objects found at lower levels. As for the temple of Zeus, the date at which it must have been completed is indicated by the Spartan dedication of a shield at the peak of one gable as a trophy of the battle of Tanagra (457); the dedication probably occurred at the Olympic festival of 456 B.C. The foundations of the temple, on the other hand, must have been laid some years later than the erection of two pedestals for sculpture buried in the process, those of a certain Praxiteles and of Onatas, which can hardly be much earlier than 480 B.C.; and on the new terrace level thus established was erected the pedestal of Smikythos, somewhere between 468 and 460 B.C.[2] These indications agree with the establishment of Elean supremacy during the 77th Olympiad = 472–468 B.C. (Herodotus, IV, 148; *cf.* Pausanias, V, 10, 2), and with the style of the sculpture which is variously dated between 475 and 455 B.C.[3] Pindar's ode of 468 B.C. (*Ol.*, VI, 1–2), alluding to the colonnaded façade of a temple, may have some connection with the new project. The bracket within which the temple must be dated, therefore, is limited by 472 and 456 B.C.,[4] and its beginning would have been in the early part of this period.

It has thus been possible to determine the archæological periods of the temples under consideration, as follows: 725–700, Heraeum at Olympia; 540–515, Peisistratid temple; 530–

[1] The theory that the external foundations belong, not to the present temple, but to an earlier structure of the same plan (Dörpfeld, *Alt-Olympia*, pp. 125–214), is to be rejected.

[2] Furtwängler, *Arch. Zeit.*, 1879, pp. 44, 151; *Bronzefunde von Olympia*, p. 5.

[3] The sculptures dated 475–465 by Schrader (*Phidias*, p. 105), 470–455 by Buschor (*Skulpturen des Zeustempels zu Olympia*, p. 14), 465–457 by Miss Richter (*Sculpture*[2], p. 41).

[4] The temple dated 468–456 by Dörpfeld (*Olympia*, II, pp. 19–21), 468–457 by Gardiner (*Olympia*, pp. 107, 234).

510, Olympieum at Athens; 530–490, old Nemesis temple at Rhamnus; 472–456, Zeus temple at Olympia; 455–435, "Theseum"; 438–413, new Nemesis temple at Rhamnus; 427–423, Nike temple; 421–413, Erechtheum.

Religion

The second factor to be considered is that of religion (*R*). This embraces two aspects, the name of the divinity worshipped in the temple, and the festival day of that divinity. The names of the divinities are, in most of the cases in question, those under which the temples are currently known. The only doubt attaches to the "Theseum" at Athens and the older temple at Rhamnus; but these may unquestionably be identified, I think, the former on topographical grounds as the temple of Hephæstus and Athena, the other on logical grounds as the predecessor and consequently older temple of Nemesis, replaced by the larger and later temple.

The four temples on the Athenian Acropolis were all dedicated to Athena in one guise or another—Athena Polias in the Parthenon, in the Peisistratid temple and in the Erechtheum, Athena Nike in the little Nike temple. The greatest festival to Athena was the Panathenaia, in connection with which her birthday was celebrated on the third day from the last of Hekatombaion, that is, Hekatombaion 27/28, in the first month of the Athenian year. Other festivals celebrated in honor of Athena at Athens will be mentioned below.

It may, I think, be regarded as certain that the Erechtheum was the lineal successor of the Peisistratid temple on the Acropolis, and hence that the same religious occasion applied to both. As for identification, it seems improbable that this was the Panathenaic festival of Athena, on Hekatombaion 27/28,[1] which seems to have been reestablished in 566 B.C. in connection with the great temple on the southern edge of the Acropolis, the ancestor of the present Parthenon; we must, however, keep this festival in mind as a possibility.

[1] The decree of Lycurgus in 335/4 B.C. (*IG.*[2], II, 334), with regard to the Panathenaic festival, mentions sacrifices at the Archaic Temple (then the Erechtheum) and the altar of Athena Hygieia, but specifies that the main sacrifices shall be to Athena Polias and Athena Nike.

Other occasions connected more intimately with the temples on the northern edge of the Acropolis are the Niketeria on Bœdromion 2, the celebration of the victory of Athena over Poseidon for the priority in Athenian cults;[1] the Kallynteria and Plynteria on Thargelion 19 and 24/25, on the latter of which the archaic image of Athena was bathed in Phaleron Bay;[2] and the Arretophoria, when the Arrephoroi carried their mysterious burden down from the Acropolis, at some time in Skirophorion.[3] The other festivals of Athena were connected with other localities and seem inappropriate for present consideration.[4]

The temple of Athena Nike might possibly be associated with the numerous victories celebrated in Bœdromion,[5] and

[1] The date is given by Plutarch (*de frat. am.*, 18; *symp. qu.*, 9, 6); the fact that it was a festival is stated by Proclus (on Plato, *Tim.*, 53d, p. 173, 9). Mommsen (*Feste der Stadt Athen*, p. 171, n. 4) argues that Bœd. 2 was a day of strife and so unsuitable for a festival, which he arbitrarily postpones to Bœd. 3, stating (*ibid.*, p. 172, n. 2) that a court session known to have been held on Bœd. 3 (*IG.*², II, 1678, line 28) would not be an objection. Diepolder (*Attische Feste*, p. 235, n. 2) is non-committal.

[2] The two festivals are usually grouped together in the definitions (Harpocration, Photius, *Etym. Mag. s.vv.*). The dates are given as Thargelion 19 and 28/29 by Photius (*s.v.*), as after the Bendidieia on Thargelion 20 by Proclus (on Plato, *Tim.*, 21a, p. 85, 28), or as Thargelion 24/25 in the case of the Plynteria by Plutarch (*Alcibiades*, 34). Mommsen (*op. cit.*, pp. 486–488, 491–504) prefers Thar. 19 and 24/25, Diepolder (*op. cit.*, pp. 17–22) Thar. 23/24 and 24/25. As both agree, the date Thar. 28/29 given by Photius must be a clerical error, and is contradicted by the fact that an assembly of the Council was held on that day in 338/7 B.C. (Æschines, 3, 27), an event which Xenophon (*Hell.*, I, 4, 12) and Plutarch (*l.c.*) both insist to be impossible on the day of the Plynteria. On the other hand, there is no foundation for Diepolder's assumption that the two festivals must have been on successive days.

[3] The month is given by *Etym. Mag.*, 149, 14. On this festival see Diepolder (*op. cit.*, pp. 9–17); Mommsen practically ignores it (*op. cit.*, p. 510, n. 1).

[4] Such were the Synoikia on Hekatombaion 16 (Mommsen, pp. 35–39; Diepolder, pp. 36–38), associated particularly with the altar of Eirene; the Oschophoria in Pyanopsion (Mommsen, pp. 282–288; Diepolder, pp. 142–147), associated with the sanctuary of Athena Skiras in Phaleron; the Chalkeia on Pyanopsion 29/30 (discussed in connection with the "Theseum"), associated with Hephæstus and his temple; the Skira on Skirophorion 12 (Mommsen, pp. 504–511; Diepolder, pp. 46–47), associated with the Eleusinian divinities and with sanctuaries of Athena Skiras at Phaleron and near the Kephisos; the Diisoteria in Skirophorion before the 20th (Mommsen, pp. 524–531; Diepolder, pp. 174–176), associated with Zeus and their sanctuaries in Peiræus; or the sacrifice to Zeus Soter and Athena Soteira on Skirophorion 29/30, in the Athenian Agora.

[5] *I.e.* Bœd. 2, the Niketeria (see above); 3 or 4, Platæa (see following note); 6, Marathon (the actual event having been Bœd. 16, see *A. J. A.*, 1934, pp. 444–445); 24/25, Salamis (for the date, *A. J. A.*, 1934, pp. 443–444).

perhaps particularly with the Battle of Platæa on Bœdromion 4,[1] an event depicted on its sculptured frieze. It would also be logical to associate it with the Panathenaic festival on Hekatombaion 27/28, when the main sacrifices were to Athena Nike and Athena Polias.[2]

The festivals which might be connected with the temple of Zeus Olympius (the Olympieum) at Athens are the Diasia, the greatest Athenian festival of Zeus, on the eighth day before the last of Anthesterion (Anth. 22/23),[3] and the Olympieia on Mounichion 19.[4] The Diasia seems less satisfactory because it was associated with Zeus Meilichios rather than with Zeus Olympius;[5] but since both were celebrated in the same region, just outside the city toward the Ilissus, both must be kept in mind.

The festival which would normally be connected with the "Theseum," now identified on archæological grounds as the temple of Hephæstus, would be the Hephæstieia,[6] of which, unfortunately, the date is not recorded. We know that the Hephæstieia existed at least as early as the time of Herodotus (VIII, 98). The festival was reorganized in 421/0 B.C., but the inscription in which the facts are recorded does not preserve the month and day.[7] Aristotle (*Ath. Pol.*, 54, 7) tells us that the Hephæstieia were made quadrennial in 329/8 B.C., this being one of the latest dates mentioned in his "Constitution of Athens." After this time, furthermore, we have no more references to the Hephæstieia. On the other hand, beginning with the third century, we begin to have numerous

[1] For the preference for Bœd. 4 over Bœd. 3, the two dates given by Plutarch, see p. 167.

[2] *IG.*², II, 334 (see note on p. 131); it should also be mentioned that the finances of Athena Nike were combined with those of Athena Polias (*IG.*², I, 324).

[3] For the date see Schol. Aristophanes, *Clouds*, 408. Also Mommsen, *Feste*, pp. 421–426; Diepolder, *Attische Feste*, pp. 155–157.

[4] The date Mounichion 19 is given by Plutarch (*Phocion*, 37) as that of a great festival of Zeus including cavalry contests; and cavalry contests were included in the Olympieia (*IG.*², II, 3079). See also Mommsen, *op. cit.*, pp. 465–468; Diepolder, *op. cit.*, p. 177.

[5] Thucydides, I, 126, 6; Schol. Aristophanes, *Clouds*, 408; *Knights*, 445; Pollux, I, 37; Suidas, *s.v.* Διάσια: *Etym. Mag.* 270, 14.

[6] Mommsen, *op. cit.*, pp. 340–342; Diepolder, *op. cit.*, pp. 212–213.

[7] *IG.*², I, 84.

references to a festival hitherto unmentioned, the Chalkeia.[1] It occurs in inscriptions of the third and second centuries, in one instance in direct association with Athena Archegetis, while in two of the others Athena is mentioned three or six lines later.[2] Also some of the ancient writers refer to it as a festival of Athena.[3] It is mentioned again, however, as a festival of Hephæstus.[4] And there seems to have been some dispute about this in antiquity, since one writer was very emphatic about its being a festival of Hephæstus and not of Athena.[5] In any case, it was generally agreed that it had originally been a great festival of all the people, and that it later became merely a festival of the artisans and metalworkers. We may infer, therefore, that the Chalkeia were the debased form of the Hephæstieia; possibly they were instituted in 329/8 B.C. to fill the three intervening years (as "lesser Hephæstieia") when the Hephæstieia were transformed from annual into quadrennial festivals; [6] perhaps they eventually replaced the Hephæstieia entirely. The important fact at present is that we know the date of the Chalkeia, the last day of Pyanopsion (Pyan. 29/30).[7]

The two successive temples of Nemesis at Rhamnus were undoubtedly associated with a festival known as the Nemesieia.[8] Unfortunately the day of this festival is not mentioned in the surviving allusions. It has been suggested,[9] however, and with great plausibility, that it was a local form

[1] See Mommsen, *op. cit.*, pp. 342–348; Diepolder, *op. cit.*, pp. 35–36; von Schoffer, in *R. E.* (Pauly-Wissowa, *Real-Encyclopadie*), III, 2067–2068.

[2] Dow, *Prytaneis*, pp. 38–39, no. 4 (of 290–275 B.C.); *IG.*², II, 674 (of 274/3 B.C.); 930; 990

[3] Suidas and Harpocration, *s.v.*; *Etym. Mag.* 805, 43; Eustathius, *Il.*, II, 552, p. 284, 35.

[4] Pollux, VII, 105.

[5] Phanodemus, quoted by Suidas and Harpocration, *s.v.*

[6] It is to be noted that 274/3 does not fall in a quadrennial year counting from 329/8 B.C

[7] For the date, see Suidas, Harpocration, and Eustathius, *ll. c.*

[8] Demosthenes, XLI, 11; Schol. on the same; Suidas, Harpocration, and Photius, *s.v.* Νεμέσια; Bekker, *Anec. gr* p. 282, 32; *BCH*, 1930, p 269, line 27 (inscription of 236/5, archonship of Ekphantos). See also Mommsen, *op. cit.*, pp 172, n. 4, 174–175, 178; Diepolder, *op. cit.*, pp. 219, 230.

[9] By Mommsen, *op. cit.*, pp 174–175.

of a similar and widespread Attic festival dedicated to the souls of the departed, the Genesia,[1] celebrated on Bœdromion 5,[2] the day before the celebration for the Battle of Marathon.

The two successive temples at Olympia, the Heræum wherein Zeus was originally worshipped, and the new temple of Zeus alone, are both to be associated with the Olympic festival itself. The main day of the festival, that devoted to the religious celebration, was the fourteenth day of the month,[3] that of the full moon. The unique feature of the Olympic festival, however, was that the successive quadrennial celebrations did not occur at the same moment of the local Elean year, but rather in alternating months, Apollonios and Parthenios. In this respect the Olympic festivals differed markedly from the Athenian, which were confined to specific months, so that the exact relationship of the recurring festivals and the intervals between them were subject to the mercy of the calendar, being matters of secondary importance to be regulated by the authorities as the year took its course.

The combination of the archæological and religious evidence obtained up to the present yields the following results:

TABLE III

ARCHÆOLOGICAL AND RELIGIOUS EVIDENCE FOR GREEK TEMPLE DATES

Older Parthenon,	Acropolis	490–480,	Hekatombaion 27,28
Peisistratid temple,	Acropolis	540–515,	Bœdromion 2, Thargelion 19 or 24,25, or Skir.
Erechtheum,	Acropolis	421–413,	Bœdromion 2, Thargelion 19 or 24 25, or Skir.
Athena Nike temple,	Acropolis	427–423,	Hekatombaion 27 28, or Bœdromion 2—24 25
Olympieum,	Athens	530–510,	Anthesterion 22 23, or Mounichion 19
"Theseum,"	Athens	455–435,	Pyanopsion 29 30
Old Nemesis temple,	Rhamnus	530–490,	Bœdromion 5
New Nemesis temple,	Rhamnus	438–413,	Bœdromion 5
Heræum,	Olympia	725–700,	Apollonios 14, or Parthenios 14
Zeus temple,	Olympia	472–456,	Apollonios 14, or Parthenios 14

[1] Herodotus, IV, 26; Phrynichus, *ed.*, p. 103 Lob.; Pollux, III, 102; Suidas and Hesychius, *s.v.* Γενέσια; Bekker, *Anec. gr.*, pp. 86, 20; 231, 19. See Mommsen, *op. cit.*, pp. 172–175; Diepolder, *op. cit.*, pp. 229–230.

[2] For the date, see Solon and Philochorus as quoted in Bekker, *Anec. gr.*, p. 86, 20.

[3] Pindar and his scholiasts, Plutarch, and Pausanias all agree.

Calendar

The next term in our equation (C) is man-made astronomy, the local calendar, which in Greece was always a lunar calendar. So far as Athens is concerned, for the greater part of the period which concerns us, down to an observation of the summer solstice marking the introduction of the nineteen-year cycles of Meton, on Skirophorion 13 = June 27, 432 B.C.,[1] according to an almanac discovered at Miletus, we are without exact information. This Athenian date in 432 B.C. evidently belonged to the new Metonic system, but might equally well have been common both to this and to the older eight-year cycles; further investigation is required before we can ascertain whether any readjustment of the days was required at this point.[2] Before this date the old eight-year cycle (*octaëteris*) of 99 months was in operation.[3] The only fixed point in the earlier period is the record of the previously mentioned solar eclipse in 480 B.C., the cause of the Spartan retirement from the Isthmus just after the battle of Salamis (Herodotus, IX, 10); the date may be reckoned as October 2 at 3 P.M. Since the battle of Salamis, a few days previously, was fought "in the twenties of Bœdromion" (Plutarch, *Camillus*, 19, 6),[4] it is evident that the eclipse must have been at the very end of Bœdromion, just before the visible new moon locating Pyanopsion 1 = October 3/4. Hence the preceding new year day, 59 days before the beginning of Bœdromion, was within a day of July 7, 480 B.C.[5]

One new year day is an insufficient basis for the definite location of a system of eight-year cycles. But it so happens that if we can accept, in agreement with all the available evidence, the above-mentioned date of laying out the axis of the Older Parthenon as Hekatombaion 27 = August 31, 488

[1] See my *Archons of Athens*, pp. 311–318, for the exact determination of the date.

[2] I formerly left this question in abeyance (*Archons*, p. 317; *A. J. A.*, 1934, p. 446).

[3] *Archons*, pp. 297–308.

[4] *Cf.* also Polyænus, III, 11, 2 (Bœd. 20); Herodotus, VIII, 65 (shortly after the Eleusinia).

[5] See my article in *A. J. A.*, 1934, pp. 443–445.

B.C.,[1] we obtain a second new year day on August 5, 488 B.C. It happens that the interval, furthermore, while covering eight years, includes only 98 lunar months, one deficient for an eight-year cycle. Further investigation shows that the sequence of the ordinary and intercalary years, if we avoid more than two ordinary years in succession, is capable of only one solution, not only for these eight years but also for the twelve years 490/89–479/8 B.C. from the Battle of Marathon to that of Platæa.[2] Furthermore, with only 98 months falling between the new year days of 488 and 480, it is evident that a division between two cycles must lie between these two dates, probably coinciding with one of the Panathenaic years 486 or 482; and of these we may prefer 486 B.C. for the reason that the rotation of the cycles backward from that year brings us to the year of the foundation of the quadrennial Panathenaic periods in 566 B.C., this being a year, moreover, in which the new year day, the day of the visible new moon, coincided with the summer solstice and so furnished an appropriate moment for a reform or initiation of a calendar.[3] For, in 566 B.C., the summer solstice occurred on June 28 at 9 P.M., and the true new moon on June 27 at 10 P.M.; so that the visible new moon (or at least the "business day" following the visible new moon) and the era date may have coincided on June 30.[4] The number of days elapsed between June 30, 566, and July 14, 486 B.C., was 29,234 days. After a series of experiments with uniform cycles of 2923 or 2924 days, and with double cycles of 5847 days (cycles alternately of 2923 and 2924 days), which it would be tedious to recapitulate here, I finally returned to a modification of the scheme which

[1] See p. 134.

[2] See *A. J. A.*, 1934, pp. 444–445.

[3] A similar moment was chosen for the reform by Kallippos in 330 B.C. (*Archons*, pp. 363–364).

[4] Allowing 41.4 hours after the time of the conjunction for the first appearance of the new moon (*Archons*, p. 300, n. 1), we reach the evening of June 29, when the lunar month actually began at sunset. But the first "business day" would then have been the following one (cf. *Archons*, pp. 299, 314–315, 365), *i.e.* June 30. The date of the summer solstice was usually 34–38 hours in error as determined by the Greek astronomers (*Archons*, pp. 311–312, 317, 318), and so could likewise fit June 30.

I published a few years ago. It would seem that the cycle was first established as 2923 days; but after three rotations it was modified (perhaps on the analogy of the Doric calendars, see below) to 2924 days, only to be restored to 2923 days by Cleisthenes after the observance of an increasing error. The initial dates of the cycles would be as follows:[1]

TABLE IV

New Year Days (Hekatombaion 1) Beginning Eight-Year Cycles, 566–432 B.C.

B.C.	Hek. 1	Cycle	B.C.	Hek. 1	Cycle
566	June 30	(2923 days)	494	July 14	(2923 days)
558	July 1	(2923 days)	486	July 15	(2923 days)
550	July 2	(2923 days)	478	July 16	(2923 days)
542	July 3	(2924 days)	470	July 17	(2923 days)
534	July 5	(2924 days)	462	July 18	(2923 days)
526	July 7	(2924 days)	454	July 19	(2923 days)
518	July 9	(2924 days)	446	July 20	(2923 days)
510	July 11	(2924 days)	438	July 21	(2186 days)
502	July 13	(2923 days)	432	July 15	(6940 days)

The determination of the beginnings of the successive eight-year cycles automatically fixes, of course, the new year day of the first year of each cycle. But for each of the seven other new year days within a cycle there are two possibilities. This results from the fact that we have no reason for assuming that the sequence of ordinary and intercalary years within each eight-year cycle was in any way preordained. Undoubtedly, as in the Metonic nineteen-year cycles that followed,[2] the intercalation proceeded at random, controlled only by the necessity of having three intercalary years within the eight, and of avoiding more than one intercalary or two ordinary years in succession.[3] Thus there are ten possible methods of

[1] This table differs slightly from the one which I had tentatively proposed (*A. J. A.*, 1934, pp. 446, 447), in the adoption of June 30 rather than June 29 as the era date; in the postponement of all the initial dates of the cycles by one day; and in the use of three uniform cycles of 2923 days at the beginning instead of varying from 2922 to 2923 and 2924 days.

[2] *Archons*, pp. 320–321.

[3] This general law of the avoidance of more than two successive ordinary years must be employed with a certain degree of caution, since it was sometimes violated under exceptional circumstances, even within the limits of a cycle, as

intercalation in such ways that there will never be more than two ordinary years together within the individual cycle, as follows: (*A*) 1, 3, 6; (*B*) 1, 4, 6; (*C*) 1, 4, 7; (*D*) 2, 4, 6; (*E*) 2, 4, 7; (*F*) 2, 5, 7; (*G*) 2, 5, 8; (*H*) 3, 5, 7; (*I*) 3, 5, 8; (*J*) 3, 6, 8. In two cases (*D, H*) the preceding and following cycles should theoretically have been differently constituted, in order to avoid a succession of three ordinary years at the points of division between the cycles. According to these various methods, the months (numbered 1–99) containing the new year days would be the following (those initiating intercalary years marked by asterisks):

TABLE V

VARIANT TYPES OF EIGHT-YEAR CYCLES

	A 1, 3, 6	*B* 1, 4, 6	*C* 1, 4, 7	*D* 2, 4, 6	*E* 2, 4, 7	*F* 2, 5, 7	*G* 2, 5, 8	*H* 3, 5, 7	*I* 3, 5, 8	*J* 3, 6, 8
I........	1*	1*	1*	1	1	1	1	1	1	1
II......	14	14	14	13*	13*	13*	13*	13	13	13
III .	26*	26	26	26	26	26	26	25*	25*	25*
IV.	39	38*	38*	38*	38*	38	38	38	38	38
V ..	51	51	51	51	51	50*	50*	50*	50*	50
VI... .	63*	63*	63	63*	63	63	63	63	63	62*
VII..	76	76	75*	76	75*	75*	75	75*	75	75
VIII.....	88	88	88	88	88	88	87*	88	87*	87*

It is apparent from this comparison that the eight new year days might have occurred in any of the following fifteen months, counting from the beginning of a cycle: 1 (*A–J*), 13 (*D–J*), 14 (*A–C*), 25 (*H–J*), 26 (*A–G*), 38 (*B–J*), 39 (*A*), 50 (*F–J*), 51 (*A–E*), 62 (*J*), 63 (*A–I*), 75 (*C, E–J*), 76 (*A–B, D*), 87 (*G, I–J*), or 88 (*A–F, H*). Thus it becomes possible, with cycles of 2923 and 2924 days, to locate the possible new year days between 566 and 432 B.C. in a sort of perpetual

in 425–421 B.C. (when the first Metonic cycle was apparently misunderstood by the archons at the end of an eight-year period—see Table VII—thereby causing a calendar irregularity which Aristophanes ridiculed, *Clouds*, 615–626, and perhaps also *Peace*, 406–415), and again in 306–303 (after the confusion due to the creation of two additional tribes and the vagaries of Demetrius Poliorcetes), and also in 248–245 B.C. (toward the end of a Metonic cycle in which intercalary years had been employed too extravagantly at the beginning).

TABLE VI

POSSIBLE NEW YEAR DAYS BETWEEN 566 AND 432 B.C.

Year no.	I	II		III		IV		V		VI		VII		VIII	
Month no.	1	13	14	25	26	38	39	50	51	62	63	75	76	87	88
System	A-J	D-J	A-C	H-J	A-G	B-J	A	F-J	A-E	J	A-I	C,E-J	A-B,D	G,I-J	A-F,H
Cycle beginning								JUNE							
566	30	19	18	8	8	27	27	16	16	5	4	24	24	13	13
558	1	20	19	9	9	28	28	17	17	6	5	25	25	14	14
550	2	21	20	10	10	29	29	18	18	7	6	26	26	15	15
542	3	22	21	11	11	30	30	19	19	8	7	27	27	16	16
534	5	24	23	13	13	2	1	21	21	10	9	29	29	18	18
526	7	26	25	15	15	4	3	23	23	12	11	1	31	20	20
518	9	28	27	17	17	6	5	25	25	14	13	3	2	22	22
510	11	30	29	19	19	8	7	27	27	16	15	5	4	24	24
502	13	2	31	21	21	10	9	29	29	17	17	6	5	25	25
494	14	3	1	22	22	11	10	30	30	18	18	7	6	26	26
486	15	4	2	23	23	12	11	1	31	19	19	8	7	27	27
478	16	5	3	24	24	13	12	2	1	20	20	9	8	28	28
470	17	6	4	25	25	14	13	3	2	21	21	10	9	29	29
462	18	7	5	26	26	15	14	4	3	22	22	11	10	30	30
454	19	8	6	27	27	16	15	5	4	23	23	12	11	1	31
446	20	9	7	28	28	17	16	6	5	24	24	13	12	2	1
438	21	10	8	29	29	18	17	7	6	25	25				

JULY

AUGUST

calendar, there being two alternatives in every case except for the first year of each cycle.[1]

In the absence of further information, it is useless to attempt to determine more accurately the positions of the individual years. For the present the problem may be left in abeyance, to be resumed after we have considered the astronomical observations on the temples.

Beginning with 432 B.C. we have numerous astronomical records which enable us to locate very definitely the beginnings of the successive Metonic cycles of nineteen years (containing 235 months or 6940 days), as follows: July 15 in 432, 413; July 16 in 394, 375, 356, and 337; the beginning of the Kallippic cycle on June 29, 330 B.C.; and so on.[2] In this period, too, we have numerous synchronisms which enable us to fix accurately the positions of many of the individual years within the cycles;[3] and this is particularly true in the period which concerns us here, the last third of the fifth century, when we have an additional check in the independent prytany calendar of the Athenian Council, which, as Meritt discovered, ran approximately on a solar basis. Within these last thirty-three years, therefore, the exact days in terms of the Julian calendar may be determined with almost perfect accuracy in the various recently published tables.[4]

[1] In the cycles with 2923 days, one year having 355 days, the extra day is inserted for convenience in the first of the two Julian leap years. In the cycles with 2924 days, two years having 355 days, the two extra days are inserted in the two Julian leap years. It is to be understood that the new year dates given in the table are subject, in some cases at least, to a readjustment of one day if we assume different locations for the 355-day years.

[2] These observations are fully discussed in my *Archons of Athens*, pp. 309–440.

[3] As a result of the discovery of many new inscriptions in the Athenian Agora, several of the details of my earlier arrangement, later than the fifth century, demand readjustment (see my forthcoming *Athenian Archon List in the Light of Recent Discoveries*).

[4] There are, to be sure, divergent systems even for this period. But Meritt's earlier arrangement (*Athenian Calendar*, pp. 118–120) has now been altered (*Athenian Financial Documents*, pp. 177–179) until it is much more in conformity with my own (*Archons*, pp. 424–425). It will be understood that any dates hereafter cited between 432 and 406 B.C. coincide both in his tables and in mine. With regard to the years 422–420 B.C., McGregor has shown good reason for reversing the sequence of the ordinary and intercalary years (*A. J. P.*, 1938, pp. 145–168); this I have accepted and incorporated in Table VII.

TABLE VII

CALENDAR OF NON-COTERMINOUS CIVIL AND PRYTANY YEARS

	Civil (Lunar) Years		Prytany (Solar) Years	
B.C.	Hek. 1	Length	Pryt. I, 1	Length
432/1	July 15	(O, 355)	June 27 = Skir. 13	(366)
431/0	July 5	(I, 384)	June 28 = Skir. 24	(365)
430/29	July 24	(O, 354)	June 28 = Skir. 5	(366)
429/8	July 12	(O, 354)	June 28 = Skir. 17	(365)
428/7	July 1	(I, 384)	June 28 = Skir. 27	(366)
427/6	July 20	(O, 354)	June 29 = Skir. 9	(366)
426/5	July 9	(I, 384)	June 30 = Skir. 21	(366)
425/4	July 27	(O, 354)	June 30 = Skir. 3	(368)
424/3	July 16	(O, 354)	July 3 = Skir. 17	(365)
423/2	July 5	(O, 354)	July 3 = Skir. 28	(365)
422/1	June 24	(O, 355)	July 3 = Hek. 10	(365)
421/0	June 13	(I, 384)	July 2 = Hek. 20	(365)
420/19	July 2	(O, 355)	July 2 = Hek. 1	(365)
419/8	June 22	(I, 384)	July 2 = Hek. 11	(365)
418/7	July 11	(O, 354)	July 2 = Skir. 21	(365)
417/6	June 29	(I, 384)	July 1 = Hek. 3	(365)
416/5	July 18	(O, 355)	July 1 = Skir. 13	(365)
415/4	July 8	(I, 384)	July 1 = Skir. 24	(364)
414/3	July 27	(O, 354)	June 30 = Skir. 4	(364)
413/2	July 15	(O, 355)	June 28 = Skir. 14	(365)
412/1	July 5	(I, 384)	June 28 = Skir. 24	((365))
411/0	July 24	(O, 354)	(June 28 = Skir. 5)	
410/09	July 13	(O, 354)		
409/8	July 1	(I, 384)		
408/7	July 20	(O, 355)		
407/6	July 10	(O, 354)		
	etc.			

The only non-Attic temples here considered are the two at Olympia, where another local form of the lunar calendar prevailed. This Elean calendar has not been definitely worked out, but may be approximated by means of the Olympic festivals. We are told by ancient authorities that the festival was celebrated in the heat of summer,[1] at the time of the full moon (the chief day being the 14th of the month),[2] and at intervals of forty-nine and fifty months alternately, in the months Apollonios and Parthenios, which late writers equated with the Egyptian months Mesori and Thoth.[3] Considering the late epoch at which these passages were written,

[1] Censorinus, 21, 6; Schol. Pindar, *Ol.* III, 35.
[2] See p. 140.
[3] Schol. (Porphyrius on) Homer, *Il.* X, 252; Schol. Pindar, *Ol.* III, 35.

the Egyptian months used for comparison would hardly be those of the shifting native Egyptian year, but rather those of the reformed Egyptian year of the Alexandrian Era as established by Augustus (23 B.C.), with Thoth 1 always coinciding with August 29, as it naturally did in the shifting Egyptian years 25–22 B.C.[1] With Thoth 1 = August 29, it is evident that the months of Mesori and Thoth, with the five (or six) intervening epagomenal days, would cover July 24/25 to September 27 inclusive. Within these limits, therefore, the Olympic festival should normally be located. The sequence of the two Olympic months was undoubtedly Apollonios and Parthenios, since both of our ancient authorities seem to equate Apollonios with Mesori (the last Egyptian month) and Parthenios with Thoth (the first month of the following Egyptian year).[2] This is the sequence now generally accepted, and, though there is a slight possibility of error—an error which would involve merely an interchange of names and so be inconsequential for our purposes—we have no reason to change. Their exact position in the Elean year is more important; and this is a matter of dispute. The Scholiast on Pindar tells us that the games were celebrated in the eighth month from the first (inclusive), which occurred at about the time of the winter solstice and was called "Thosythias," a name which has been variously emended to Diosthyos[3] or Theiluthios.[4] Consequently the earlier investigators began the Elean year at the winter solstice with "Thosythias."[5] Later authorities, however, have disregarded this evidence, noting that Apollonios and Parthenios fell at a time of the year when they would be approximately

[1] The reform occurred in the fifth year of Augustus (Theon of Alexandria, *Comment*, p. 332, Bâle, 1538; cf. Panodoros, in Syncellus, 313 Par.), which, if counted from 27 B.C., would bring us to 23 B.C. when the new year naturally fell on August 29 and would require no readjustment. Attempts to date the calendar reform in 25 B.C., or even in 30 B.C. (Ginzel, *Chron.* I, pp. 224–228), demand an adjustment which it is practically impossible to explain.

[2] The Scholiast on Pindar names them in this order. But the Scholiast on Homer reverses the order, and likewise consistently reverses the order of the two Egyptian months, so that the equivalence is maintained.

[3] Böckh, *C. I. G.* II, p. 370; Bischoff, in *R. E.*, X, 1589 (retracted in text).

[4] Dittenberger, in *R. E.*, V, 1146; Bischoff, in *R. E.*, X, 1577.

[5] Unger, in *Mullers Handbuch*, I², 1892, p. 774.

equivalent to the Attic Metageitnion and Bœdromion, and so assuming that Apollonios and Parthenios were likewise the second and third months of the Elean year, of which the beginning, therefore, would occur at the summer rather than at the winter solstice.[1] It seems to me that this more recent view is unfounded and contrary to the evidence, and that it fails to explain the unique alternation of the two Olympic months. Only if the festival occurred in the latter part of the year, literally following the tradition that it should occur in the eighth month, would we have a logical explanation, namely, that the alternation was caused by the presence or absence of the intercalary month (which would presumably have occupied its normal position at the middle of the year).[2] This means that we are to follow literally the Scholiast on Pindar, and to regard the eighth month of the year as the Olympic month, whether the year be ordinary or intercalary. Consequently we must regard Apollonios as the seventh month in ordinary years and Parthenios as the eighth; Apollonios would have been the Olympic month only in intercalary years, Parthenios only in ordinary years. This forces us to assume that, for the purpose of maintaining the regular distribution of the festivals and the alternating intervals of forty-nine and fifty months, the calendar was more carefully regulated at Olympia than at Athens. Either the first year of the eight-year cycle was always intercalary (festival in Apollonios) and the fifth ordinary (festival in Parthenios), the cycle being one of the three systems *A*, *B*, or *C*; or the first year was always ordinary (festival in Parthenios) and the fifth intercalary (festival in Apollonios), the cycle being one of the four systems *F*, *G*, *H*, or *I*.[3] Furthermore, it would be impossible to assume that the shorter interval of forty-nine months filled the interval from Apollonios (intercalary year) to Parthenios (ordinary year), since this would invariably bring three ordi-

[1] Weniger, *Klio*, 1904, pp. 125–151; 1905, pp. 1–38, 184–218; 1906, pp. 19–20; Nissen, *Orient*, pp. 182–195; Bischoff, in *R. E.*, X, 1577, 1589.

[2] Fotheringham (*Journ. Hellenic Studies*), 1919, pp. 177–178; 1925, p. 83) also attributes this variation—which he regards as an irregular one—to the intercalary month

[3] See p. 137.

nary years together. Hence we must assume that there were fifty months from Apollonios to Parthenios, and forty-nine from Parthenios to Apollonios.

Within the fifth century, our most important evidence is the fact that in 480 B.C. the Olympic festival practically coincided with the Battle of Thermopylae. For, speaking of preparations for the defense, Herodotus (VII, 206) tells us that "an Olympic festival fell due at the same time as all these doings; wherefore they (the Peloponnesians) sent their advance guard, not supposing that the war at Thermopylae would so speedily come to an issue." And in particular "the Spartans proposed that later when they should have kept the feast of the Karneia, which was their present hindrance, they would leave a garrison at Sparta and march out with the whole of their force and with all speed." When the Persians questioned Arcadian deserters as to what the Greeks were doing, "the Arcadians told them that the Greeks were keeping the Olympic festival and viewing sports and horse races" (VIII, 26). Later, in speaking of the preparations for the Battle of Salamis, we are told that "the Olympic and Karneian festivals were now past" (VIII, 72). The Battle of Salamis, as we have seen, occurred about Bœdromion 24/25 (Sept. 27/28).[1] The Battle of Thermopylæ had occurred some time previously, no later than Metageitnion (Aug. 6–Sept. 3). The Olympic festival must have coincided, therefore, with the full moon of August 19; for the preceding moon would have anticipated the earliest allowable date July 24/25 = Mesori 1. This location of the Olympic games in 480 B.C. at the middle of the Attic month Metageitnion is in agreement also with the inference from Herodotus that they were held in that year at about the same time as the Karneia; for the latter fell in the Spartan month Karneios, which Plutarch (*Nicias*, 28) equates with the Attic month Metageitnion.[2]

[1] See p. 133.

[2] It is true that, owing to the diverse methods of intercalation in Attica and the Peloponnesus, Karneios must on some occasions have been equivalent to Bœdromion, and on others even to Hekatombaion (Meritt, *Class. Phil.*, 1931, p. 83). But in 480 B.C. the association with the Olympic festival requires the normal equivalence with Metageitnion.

Thus all the evidence combines to show that in 480 B.C. we have a fixed point in the Elean calendar, in that the Olympic festival fell on the full moon of August 19, the earlier of the two full moons occurring within the allowable period July 24/25–September 27. It is only reasonable to assume, furthermore, that this was likewise the earlier of the two Olympic months, Apollonios, and consequently that this was an intercalary year in the Elean system. This agrees with the evidence that the fifty months were reckoned from Apollonios to Parthenios, the forty-nine from Parthenios to Apollonios, showing that a festival in Apollonios was always in the earlier part of the period, one in Parthenios in the later part. Consequently, with the knowledge that Apollonios 14 in 480 B.C. coincided approximately with the full moon of August 19, or rather with the following "business day," we may work out the following tentative arrangement for the next few festivals (assuming for the moment that the eight-year cycles were alternately of 2923 and 2924 days):[1]

480, August 19	= Apollonios 14	
		interval 50 months, 1476 days
476, September 3	= Parthenios 14	
		interval 49 months, 1447 days
472, August 20	= Apollonios 14	
		interval 50 months, 1477 days
468, September 5	= Parthenios 14	
		interval 49 months, 1447 days
464, August 22	= Apollonios 14	
		interval 50 months, 1476 days
460, September 6	= Parthenios 14	
	etc.	

Astronomy

The last term of our equation (*As*) is the astronomy of nature, and its reflection in the axial orientation of the tem-

[1] For convenience, the year 480 is here assumed to mark the beginning of a cycle of 2923 days; but it could also mark the beginning of a cycle of 2924 days, or the middle of a cycle of 2923 or 2924 days. The extra day in three out of four quadrennial periods is located for convenience in the Julian leap year. It will be seen that there are several possibilities of minor adjustment, as well as a more general adjustment in case it should be ascertained that the months were not running exactly true to the moon.

ples. The investigation comprises the analysis of two sets of coordinates, horizontal and vertical, amplitudes and altitudes.

For most of the temples now to be examined, astronomical observations of the directions of their axes were obtained with such care by Penrose that it would be superfluous to attempt to attain greater accuracy. These orientations are shown in the first column of Table VIII, in the form of azimuths measured clockwise from the south, and, in the second column, of amplitudes measured north or south of true east. The only instance in which I have had to reject Penrose's result is in the temple of Athena Nike, for which he gives 275° 43′ 27″; but this was based on the modern condition of the building, as distorted by the settlement of the foundations, and must be corrected with reference to the Propylaea, which Penrose himself had oriented with extreme accuracy. These orientations become, according to the theory which we are investigating, the apparent azimuths of the sun when it rises in the lines of the temple axes.

TABLE VIII

Apparent Azimuths, Amplitudes, Altitudes, and Corrections

Temples		Orientation = Apparent Sun Azimuth	Apparent Amplitude of Sun	True Horizon Altitude	Apparent Altitude of Sun's Top	True Altitude	
						of Sun's Top	of Sun's Center
Parthenon,	Acropolis	257° 7′	12° 53′ N	2° 50′	2° 52′ − 15′	= 2° 37′	2° 21′
Peisistratid,	Acropolis	260° 55′	9° 5′ N	3° 21′	3° 23′ − 13′	= 3° 10′	2° 54′
Erechtheum,	Acropolis	265° 9′	4° 51′ N	3° 37′	3° 39′ − 12′	= 3° 27′	3° 11′
Nike,	Acropolis	275° 26′	5° 26′ S	4° 56′	4° 58′ − 10′	= 4° 48′	4° 32′
Olympieum,	Athens	270° 5′	0° 5′ S	4° 39′	4° 41′ − 11′	= 4° 30′	4° 14′
"Theseum,"	Athens	283° 6′	13° 6′ S	5° 39′	5° 41′ − 9′	= 5° 32′	5° 16′
Old Nemesis,	Rhamnus	268° 30′	1° 30′ N	1° 22′	1° 24′ − 22′	= 1° 2′	0° 46′
New Nemesis,	Rhamnus	271° 25′	1° 25′ S	2° 0′	2° 2′ − 18′	= 1° 44′	1° 28′
Heraeum,	Olympia	266° 14′	3° 46′ N	1° 40′	1° 42′ − 20′	= 1° 22′	1° 6′
Zeus,	Olympia	262° 38′	7° 22′ N	2° 4′	2° 6′ − 18′	= 1° 48′	1° 32′

In the third column of Table VIII are shown the vertical angles observed with reference to the horizon in the lines of the temple axes, in every case rising above the level of the eye of the observer. Such angles may be measured in two ways, either by direct observation with a theodolite on the

spot, or, in the case of the Attic temples, by calculations on the basis of the excellent Curtius-Kaupert maps of Attica (Fig. 6). Penrose, to whom we owe the orientations, had likewise observed these altitude angles on the spot; but, for the reason that he frequently altered his observations to

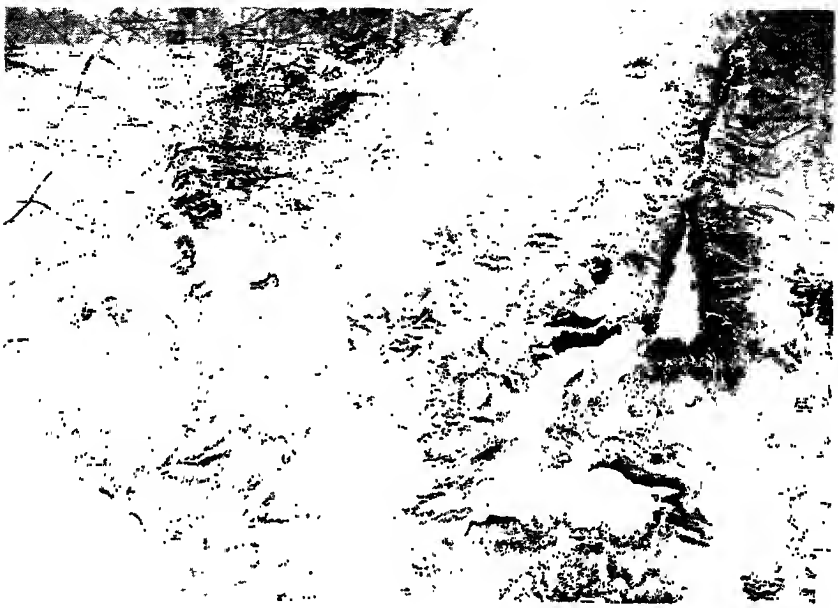

FIG. 6. Map of Athens with temple axes prolonged.

accord with the calculated path of the sun on a morning indicated by the heliacal rising (or setting) of an arbitrarily selected star, I have checked his results wherever possible by means of maps.[1] These horizon altitudes become the appar-

[1] For the Parthenon, see p. 120. For the Peisistratid temple, Penrose gives altitude 2° 40′, but this is for a calculated amplitude + 12° 16′, declination + 11° 20′; instead, on the Curtius-Kaupert maps the axis passes the crest of Mt. Hymettus at a distance of 8800 m and a height of 670 m above the sea, about 515 m above the temple platform; the angle 515 : 8800 is 3° 21′. Similarly, in the Erechtheum, Penrose gives altitude 3° 25′, but this is for a calculated amplitude + 7° 20′, declination + 7° 34′; but on the Curtius-Kaupert maps the axis passes the crest at a distance of 8550 m and a height of 695 m above the sea, about 540 m above the temple platform; the angle 540 : 8550 is 3° 37′. As for the temple of Athena Nike, Penrose's observation of an altitude of 5° 22′, while

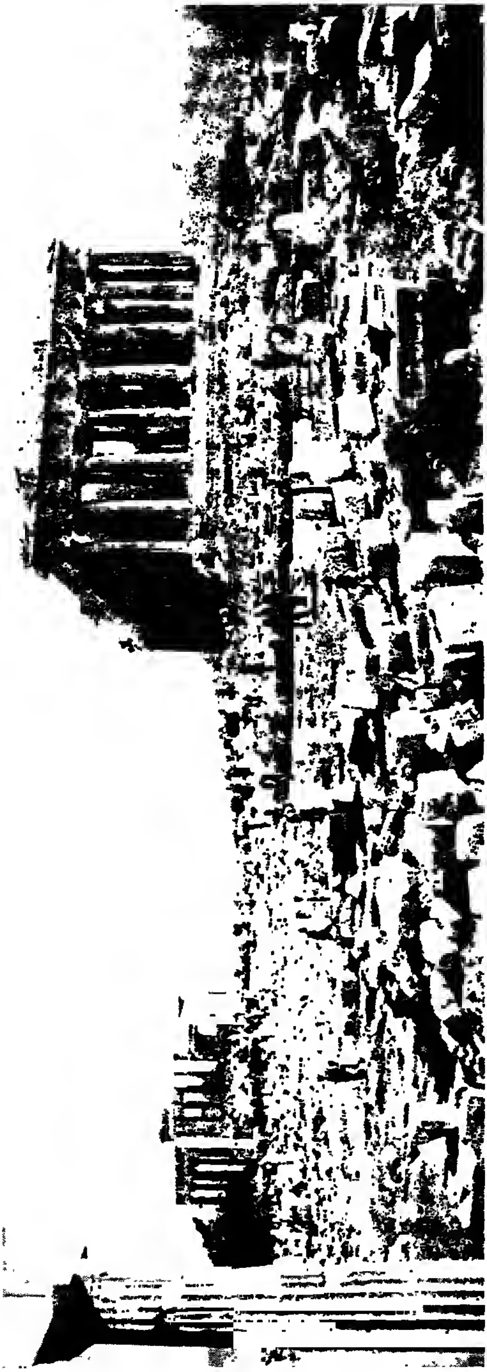

FIG. 7. Erechtheum and Parthenon, with eastern horizon.

ent altitudes of the sun as it rises in the lines of the temple axes.

These altitudes, however, are subject to various corrections. We may assume that the observations were made when the mere cap or upper limb of the sun, to the extent of about one eighth of its radius or 0° 2′, was apparent above the horizon, this being the moment proposed by Lockyer and that which I in turn observed on the line of the Parthenon; thus the apparent top of the sun should be 0° 2′ higher than the observed altitude of the horizon, as corrected in the fourth column of Table VIII. Next, in the fifth column, we note the corrections due to refraction, in order to obtain the true altitude of the sun's top. Since these refractions at low altitudes are not ordinarily included in navigation tables, which are prepared for altitudes greater than 5°, it seems desirable to reproduce Table IX which I prepared to fill the deficiency,[1] including the corrections for refraction up to 6° (the results being 9″ less than the true refraction in order to include parallax, which need not, therefore, be further considered):[2]

not altered to fit his special theory, was taken in a slightly erroneous direction; on the maps the axis at the proper orientation passes the crest at a distance of 8400 m and a height of 870 m above the sea, about 725 m above the temple platform; the angle 725 : 8400 is 4° 56′. For the Olympieum, Penrose gives altitude 4° 31′; but on the maps the axis passes the crest of Mt. Hymettus at a distance of 7500 m and a height of 690 m above the sea, and so about 610 m above the temple platform; the angle 610 : 7500 is 4° 39′. For the "Theseum," Penrose gives altitude 5° 6′, but this is for a calculated amplitude − 10° 46′, declination − 5° 17′; on the maps, however, the axis would pass the crest of Mt. Hymettus at a distance of 8800 m and a height of 940 m above the sea, and so about 870 m above the temple platform; the angle 870 : 8800 is 5° 39′. For the two temples at Rhamnus, and for the two at Olympia, Penrose's altitudes seem to be satisfactory.

[1] If I have included in my discussion an undue proportion of the methods on which the results are based, it is for the purpose of making them available to archæologists—to some of whom the processes may be as unfamiliar as they were to myself—in the hope that additional data may be accumulated in this field.

[2] The amounts for the full degrees, and those for every 5′ above 5°, are taken from Bowditch (*American Practical Navigator*, 1936, pp. 600–601); the intermediate amounts between the full degrees are found by interpolation. These are calculated for 30 in. barometric pressure and 50° Fahrenheit, no account being taken of minute variations, which would be 1/46th less for each additional 10° of temperature.

TABLE IX

REFRACTION CORRECTIONS FOR LOW ALTITUDES

	0°	1°	2°	3°	4°	5°
0′	36′ 20″	24′ 45″	18′ 17″	14′ 16″	11′ 35″	9′ 43″
5′	35′ 3″	23′ 4″	17′ 53″	14′ 0″	11′ 24″	9′ 35″
10′	33′ 51″	23′ 25″	17′ 30″	13′ 44″	11′ 13″	9′ 27″
15′	32′ 43″	22′ 48″	17′ 8″	13′ 29″	11′ 3″	9′ 20″
20′	31′ 38″	22′ 12″	16′ 46″	13′ 15″	10′ 53″	9′ 12″
25′	30′ 36″	21′ 38″	16′ 25″	13′ 1″	10′ 43″	9′ 5″
30′	29′ 38″	21′ 5″	16′ 5″	12′ 48″	10′ 34″	8′ 58″
35′	28′ 43″	20′ 34″	15′ 45″	12′ 35″	10′ 25″	8′ 51″
40′	27′ 50″	20′ 4″	15′ 26″	12′ 22″	10′ 16″	8′ 44″
45′	27′ 0″	19′ 35″	15′ 8″	12′ 10″	10′ 7″	8′ 38″
50′	26′ 13″	19′ 8″	14′ 50″	11′ 58″	9′ 59″	8′ 31″
55′	25′ 28″	18′ 42″	14′ 33″	11′ 46″	9′ 51″	8′ 25″

Finally, in the last column of Table VIII, is shown the true altitude of the center of the sun, 0° 16′ lower than the true altitude of the top.

With the aid of these two coordinates, the amplitude (or azimuth) of the center of the sun's disk, and its true altitude above the theoretical horizon, we are enabled to calculate the exact position of the path of the sun as it intersected the horizon on its diagonal ascent from north to south, and thus to ascertain its relation to the positions of the sun at equinoxes or solstices. This may be done either by computation or in graphic form. For the computation, we employ the differential formula which is particularly useful for such low altitudes:

$$d\alpha = \frac{\tan \phi}{\cos \alpha} dh, \tag{2}$$

wherein α is the (apparent) amplitude, ϕ the latitude, d the difference between true and apparent in each case, and h the height or altitude of the center of the sun's disk. The latitudes (ϕ) of the three sites under consideration are the following: Athens, 37° 58′ 20″; Rhammus, 38° 13′; Olympia, 37° 38′.

With the various terms thus ascertained, it becomes possible to obtain the true amplitude (or azimuth) of the sunrise in connection with each temple, as, for instance, in the case of the Parthenon:

$$(2) \quad d\alpha = \frac{0.7805}{0.9748} \times 141' = 0.8007 \times 141' = 113' = 1^\circ\, 53'.$$

Then, the apparent amplitude being $12^\circ\, 53'$ north, the true amplitude becomes $12^\circ\, 53' + 1^\circ\, 53' = 14^\circ\, 46'$ north, and the azimuth $255^\circ\, 14'$. Similarly, the true azimuths and amplitudes of the other temples are worked out in the first and second columns of Table XII.

The amplitude and declination are related to each other in accordance with the formula

$$(1) \qquad \sin \alpha = \frac{\sin \delta}{\cos \phi}, \qquad \text{or} \qquad \sin \delta = \sin \alpha \cdot \cos \phi,$$

wherein δ is the declination or distance of the sun's center from the celestial equator, and α is the true amplitude. Thus, again in the case of the Parthenon, we should have

$$\sin \delta = 0.2549 \times 0.7885 = 0.2010,$$
$$\delta = 11^\circ\, 36'.$$

In this way are worked out the declinations of the sun as shown in the third column of Table XII.

In order to proceed further, we must ascertain the amplitudes and declinations of sunrise throughout the various seasons of the year, at Athens in the sixth and fifth centuries B.C. Our first requisites are the dates of the solstices, the extreme limits of the solstitial arc. For the period with which we are concerned the summer solstice dates are the following: [1]

[1] For the century between 500 and 401 B.C. these are derived from Ginzel's tables (*Chron.*, II, pp. 578–579), by counting the days from midnight to midnight rather than from noon to noon, transforming the tenths of days into hours, and adding 1 hr 35 m for the difference between Greenwich and Athens. The solstices earlier than 500 B.C. have been computed for this table.

TABLE X

June Solstice Dates at Athens, 550 to 401 B.C.

550 J 30, 4 A.M.	520 J. 29, 11 A.M.	490 J 29, 4 P.M.	460 J 28, 9 P.M.	430 J 29, 4 A.M.
549 J 29, 9 A.M.	519 J 29, 4 P.M.	489 J 28, 9 P.M.	459 J 29, 4 A.M.	429 J 28, 9 A.M.
548 J. 29, 4 P.M.	518 J 29, 9 P.M.	488 J 29, 4 A.M.	458 J. 29, 9 A.M.	428 J 28, 4 P.M.
547 J 29, 9 P.M.	517 J 29, 4 A.M.	487 J. 29, 9 A.M.	457 J. 28, 4 P.M.	427 J 28, 9 P.M.
546 J. 30, 2 A.M.	516 J 29, 9 A.M.	486 J 29, 2 P.M.	456 J 28, 9 P.M.	426 J. 29, 2 A.M.
545 J. 29, 9 A.M.	515 J 29, 2 P.M.	485 J 28, 9 P.M.	455 J 29, 2 A.M.	425 J 28, 9 A.M.
544 J 29, 2 P.M.	514 J. 29, 9 P.M.	484 J 29, 2 A.M.	454 J 29, 9 A.M.	424 J 28, 2 P.M.
543 J 29, 9 P.M.	513 J. 29, 2 A.M.	483 J 29, 9 A.M.	453 J 28, 2 P.M.	423 J 28, 9 P.M.
542 J 30, 2 A.M.	512 J. 29, 9 A.M.	482 J 29, 2 P.M.	452 J 28, 9 P.M.	422 J 29, 2 A.M.
541 J. 29, 9 A.M.	511 J 29, 2 P.M.	481 J 28, 9 P.M.	451 J 29, 2 A.M.	421 J 28, 9 A.M.
540 J 29, 2 P.M.	510 J. 29, 6 P.M.	480 J 29, 2 A.M.	450 J 29, 6 A.M.	420 J. 28, 2 P.M.
539 J 29, 6 P.M.	509 J 29, 2 A.M.	479 J 29, 6 A.M.	449 J 28, 2 P.M.	419 J. 28, 6 P.M.
538 J 30, 2 A.M.	508 J 29, 6 A.M.	478 J 29, 2 P.M.	448 J 28, 6 P.M.	418 J 29, 2 A.M.
537 J 29, 6 A.M.	507 J. 29, 2 P.M.	477 J 28, 6 P.M.	447 J 29, 2 A.M.	417 J. 28, 6 A.M.
536 J 29, 2 P.M.	506 J 29, 6 P.M.	476 J 29, 2 A.M.	446 J 29, 6 A.M.	416 J 28, 2 P.M.
535 J 29, 6 P.M.	505 J 29, 2 A.M.	475 J 29, 6 A.M.	445 J 28, 2 P.M.	415 J. 28, 6 P.M.
534 J. 29, 11 P.M.	504 J 29, 6 A.M.	474 J 29, 11 A.M.	444 J 28, 6 P.M.	414 J 28, 11 P.M.
533 J 29, 6 A.M.	503 J 29, 11 A.M.	473 J 28, 6 P.M.	443 J 28, 11 P.M.	413 J. 28, 6 A.M.
532 J 29, 11 A.M.	502 J 29, 6 P.M.	472 J 28, 11 P.M.	442 J 29, 6 A.M.	412 J 28, 11 A.M.
531 J 29, 6 P.M.	501 J 28, 11 P.M.	471 J 29, 6 A.M.	441 J 28, 11 A.M.	411 J. 28, 6 P.M.
530 J. 29, 11 P.M.	500 J 29, 6 A.M.	470 J 29, 11 A.M.	440 J 28, 6 P.M.	410 J 28, 11 P.M.
529 J 29, 6 A.M.	499 J 29, 11 A.M.	469 J 28, 6 P.M.	439 J 28, 11 P.M.	409 J 28, 6 A.M.
528 J 29, 11 A.M.	498 J 29, 4 P.M.	468 J 28, 11 P.M.	438 J 29, 4 A.M.	408 J 28, 11 A.M.
527 J 29, 4 P.M.	497 J 28, 11 P.M.	467 J 29, 4 A.M.	437 J 28, 11 A.M.	407 J. 28, 4 P.M.
526 J 29, 11 P.M.	496 J 29, 4 A.M.	466 J 29, 11 A.M.	436 J 28, 4 P.M.	406 J. 28, 11 P.M.
525 J 29, 4 A.M.	495 J 29, 11 A.M.	465 J 28, 4 P.M.	435 J 28, 11 P.M.	405 J 28, 4 A.M.
524 J 29, 11 A.M.	494 J 29, 4 P.M.	464 J 28, 11 P.M.	434 J 29, 4 A.M.	404 J. 28, 11 A.M.
523 J. 29, 4 P.M.	493 J 28, 11 P.M.	463 J 29, 4 A.M.	433 J 28, 11 A.M.	403 J. 28, 4 P.M.
522 J 29, 11 P.M.	492 J 29, 4 A.M.	462 J 29, 9 A.M.	432 J 28, 4 P.M.	402 J. 28, 11 P.M.
521 J 29, 4 A.M.	491 J 29, 11 A.M.	461 J 28, 4 P.M.	431 J 28, 11 P.M.	401 J. 28, 4 A.M.

It will be noted that, while the solstice dates vary from June 28 to June 30, the central date is June 29, this being exactly true of the year 500 B.C., which may be taken as a convenient standard date. As noted above, furthermore, the obliquity of the ecliptic at this date would have been 23° 45½′, which would also have been the true maximum declination at the time of sunrise on the solstices. The corresponding amplitude at Athens, as derived from formula (1), would have been 30° 44′. On the basis of these maximum figures the sunrise amplitudes and declinations at Athens, for every day of the standard year 500 B.C., may be calculated as follows:[1]

[1] A similar table of amplitudes and declinations was published by Nissen (*Templum*, pp. 242–243, 246; and in more contracted form in *Orientation*, p. 260). These are unsuitable for our purpose, however, since the declinations were calculated for the latitude of Berlin and with an obliquity of only 23° 44′ which is suitable rather for 250 B.C.; and the azimuths of sunrise, while calculated for Athens, are based on this later obliquity and also are readjusted to allow for refraction at the theoretical horizon, thus requiring a double correction before they can be utilized.

TABLE XI

Sunrise Amplitudes and Declinations at Athens, 500 B.C.

North		South		Amplitude	Dif.	Declination	Dif.
Mar. 28	Sept. 30		Mar. 28	0° 0′	153½′	0° 0′	121′
Apr. 2	Sept. 25	Oct. 5	Mar. 23	2° 33½′	152½′	2° 1′	120′
Apr. 7	Sept. 20	Oct. 10	Mar. 18	5° 6′	150′	4° 1′	118′
Apr. 12	Sept. 15	Oct. 15	Mar. 13	7° 36′	147′	5° 59′	115½′
Apr. 17	Sept. 10	Oct. 20	Mar. 8	10° 3′	144′	7° 54½′	112½′
Apr. 22	Sept. 5	Oct. 25	Mar. 3	12° 27′	139½′	9° 47′	109′
Apr. 27	Aug. 31	Oct. 30	Feb. 27	14° 46½′	134½′	11° 36′	104½′
May 2	Aug. 26	Nov. 4	Feb. 22	17° 1′	128′	13° 20½′	98½′
May 7	Aug. 21	Nov. 9	Feb. 17	19° 9′	120′	14° 59′	92½′
May 12	Aug. 16	Nov. 14	Feb. 12	21° 9′	112′	16° 31½′	85½′
May 17	Aug. 11	Nov. 19	Feb. 7	23° 1′	102½′	17° 57′	78′
May 22	Aug. 6	Nov. 24	Feb. 2	24° 43½′	92½′	19° 15′	70′
May 27	Aug. 1	Nov. 29	Jan. 28	26° 16′	79½′	20° 25′	60′
June 1	July 27	Dec. 4	Jan. 23	27° 35½′	66½′	21° 25′	49½′
June 6	July 22	Dec. 9	Jan. 18	28° 42′	54′	22° 14½′	39′
June 11	July 17	Dec. 14	Jan. 13	29° 34′	38½′	22° 53½′	28½′
June 17	July 11	Dec. 19	Jan. 8	30° 12½′	23½′	23° 22′	17½′
June 23	July 5	Dec. 24	Jan. 3	30° 36′	8′	23° 39½′	6′
June 29		Dec. 29		30° 44′		23° 45½′	

On the basis of the foregoing table, the orientation dates of the temples may now be obtained in terms of the situation at 500 B.C. In the Parthenon, for instance, the axis fits the sunrises 126 days apart, 63 days before and after the summer solstice; in the Peisistratid temple, 140 days apart or 70 days before and after; in the Erechtheum, 156 days apart or 78 days before and after; in the Nike temple, 194 days apart or 97 days before and after; in the Olympieum, 174 and 87 days respectively; in the "Theseum," 222 and 111 days respectively; in the older temple at Rhamnus, 178 and 89 days, in the later temple 188 and 94 days respectively; at Olympia, 168 and 84 days in the Heraeum and 152 and 76 days in the temple of Zeus.[1] These results are incorporated in the last two columns of Table XII.

[1] Nissen's calculations vary only slightly from mine, giving 24 days from the equinox or 69 days from the solstice for the Peisistratid temple, 17 days from the Parthenon date and so 80 days from the summer solstice for the Erechtheum, one or two days from the equinox and so 91/92 days from the solstice for the Nike

TABLE XII

TRUE AZIMUTHS, AMPLITUDES, DECLINATIONS, AND CORRESPONDING DAYS

		True Sun Azimuth	True Sun Amplitude	True Sun Declination	Date Before	Date After
					Summer Solstice	
Parthenon,	Acropolis	255° 14′	14° 46′ N	+11° 36′	Apr. 27	Aug. 31
Peisistratid,	Acropolis	258° 37′	11° 23′ N	+ 8° 57′	Apr. 20	Sept. 7
Erechtheum,	Acropolis	262° 39′	7° 21′ N	+ 5° 47′	Apr. 12	Sept. 15
Nike,	Acropolis	271° 55′	1° 55′ S	− 1° 30′	Mar. 24	Oct. 4
Olympieum,	Athens	266° 47′	3° 13′ N	+ 2° 32′	Apr. 3	Sept. 24
"Theseum,"	Athens	279° 6′	9° 6′ S	− 7° 10′	Mar. 10	Oct. 18
Old Nemesis,	Rhamnus	267° 54′	2° 6′ N	+ 1° 39′	Apr. 1	Sept. 26
New Nemesis,	Rhamnus	270° 16′	0° 16′ S	− 0° 13′	Mar. 27	Oct. 1
Heræum,	Olympia	265° 23′	4° 37′ N	+ 3° 39′	Apr. 6	Sept. 21
Zeus,	Olympia	261° 26′	8° 34′ N	+ 6° 47′	Apr. 14	Sept. 13

The graphic form of investigation is illustrated in Fig. 8. Here the center of the horizon is placed at true east, and ten suns are represented as rising simultaneously in their proper positions on the lines of the axes of the ten temples, either over the crest of Mt. Hymettus near Athens, or over the lower hills at Rhamnus and Olympia. Both the refracted and the true positions of the sun are shown in each case, and the path of the sun is carried down to the theoretical horizon where the intersection may be compared with a scale of days measured north and south from the equinoxes. The sun for the Nike temple is given a shady character for reasons that will appear below.

The Peisistratid temple might be associated, as we have seen, with the Panathenaia on Hekatombaion 27/28, or the Niketeria on Bœdromion 2, or the Kallynteria or Plynteria on Thargelion 19 or 24/25, or the Arretophoria in Skiro-

temple, 14 days from the equinox or 79 days from the solstice for the Olympieum, 14 days from the equinox or 107 days from the solstice for the "Theseum," 4 days from the equinox and 89 days from the summer solstice for the old temple at Rhamnus and one day and 94 days respectively for the new temple, 10 days from the equinox and 83 days from the solstice for the Heræum at Olympia, 17/18 days from the equinoxes and 75/76 days from the solstice for the temple of Zeus at Olympia.

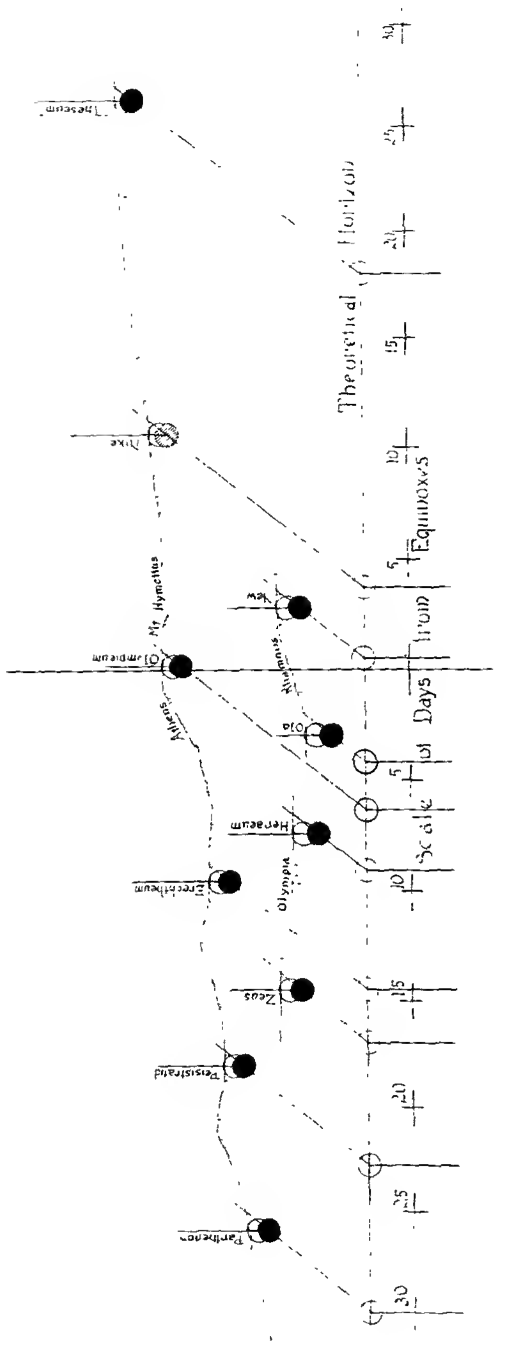

FIG. 8. Graphic investigation of axial sunrise.

phorion, the Julian dates being April 20 or September 7, within the years 540–515 B.C. The new year days *preceding* Hek. 27/28 = Sept. 7 or Bœd. 2 = Sept. 7 would necessarily be August 11/12 or July 9 respectively. Those *following* Thar. 19 = April 20 or Thar. 24/25 = April 20 would be May 30 or May 25/26 respectively. Any date in Skirophorion falling on April 20 would locate the *following* new year between April 21 and May 20 inclusive. But a glance at Table VI shows that a new year day as early as April or May is impossible, and also that a day as late as August 11/12 would be impossible before 483 B.C. (or rather, since in 483 the new year fell on July 11, we could not go back of 475 B.C.). Of the above-mentioned possibilities, therefore, we are limited to July 9, which within the allowable quarter of a century would be possible only in 537 (VI), 529 (VI), 526 (I), 521 (VI), or 518 (I), of which the first, third, and fourth would present discrepancies of two days, while the second and fifth would fit exactly. The fifth date (518) may be eliminated, however, since it is too late for the beginning of work of which the later stages seem stylistically earlier than 520 B.C. The second date (529), however, exactly fits the archæological evidence for a year shortly after 530; hence we may accept the date as 529 B.C., with the new year falling on July 9, and the temple laid out at the Niketeria festival of September 7.

The Erechtheum, therefore, should be associated with the same occasion, the Niketeria [1] on Bœdromion 2, the axis of the temple, however, indicating the Julian day September 15. The preceding new year day would then be July 17. Within the allowable period 421–413 B.C. this day would fit only the year 416 B.C., when the new year is estimated to have fallen on July 18.[2] It seems more probable, however, in view of the fact that the Erechtheum exactly encased an earlier shrine founded after the departure of the Persians in 479 B.C., and followed exactly the orientation of this earlier shrine, that the latter was the determining factor. The earliest available cele-

[1] Penrose had likewise associated the Niketeria with the Erechtheum.
[2] Tables by Dinsmoor and Meritt agree.

bration of the Niketeria, after the Persian menace was removed, would be that of Bœdromion 2 in 478 B.C., which happened also to be almost exactly the first anniversary of the Battle of Plataea (Bœd. 4, 479). And in 478 B.C. the new year day is estimated to have been July 16, with a discrepancy of one day, within the permissible margin of error; the next available moment would be 470 B.C. when the coincidence would be exact (July 17), but a delay of nine years in housing the statue of Athena within a temporary shrine is inconceivable. We may, therefore, accept the date of laying out the axis of the Erechtheum on September 15, 478 B.C. (retaining the new year, since the solstice fell on June 29, as of July 16 by assuming that the year had 355 days, or 60 days in the first two months).

The temple of Athena Nike, as we have seen, might be associated with the Panathenaia on Hekatombaion 27/28, or with the victory dates in Bœdromion (Bœd. 2 = Niketeria, 4 = Plataea, 6 or 16 = Marathon, 24/25 = Salamis),[1] the Julian day according to the orientation being October 4, and the year 427 or 426 B.C. But the Panathenaic festival would demand a new year day on September 7/8, which is impossibly late. Festivals between Bœdromion 2 and 25 would demand a new year day between July 13 and August 5 inclusive, which might fit 427 (Hek. 1 = July 20) but not 426 B.C. (Hek. 1 = July 9).[2] But in 427 B.C. October 4 was Bœdromion 18, two days from the actual and twelve days from the observed anniversary of the Battle of Marathon, so that the connection is unsatisfactory. A glance at the map (Fig. 9) will show, furthermore, that such a discussion is purely academic, since the prolonged axis of the Nike temple passes directly through the Parthenon, which was built earlier, so that the crest of Mt. Hymettus was never actually visible along this axis. Even more, a Mycenaean fortification wall 6.00 m (19 ft 8 in) thick stands directly in front of the Nike temple and

[1] Nissen associated the Nike temple with the Niketeria or one of the other victory festivals in Bœdromion.

[2] Tables by Dinsmoor and Meritt agree.

effectually cuts off any view of the natural horizon.[1] This Mycenaean wall stood throughout classical times, and its effectiveness as an obstacle can be gauged by the fact that the adjoining corner of the marble Southwest Wing of the Propylaea is beveled off from ground to roof to fit against

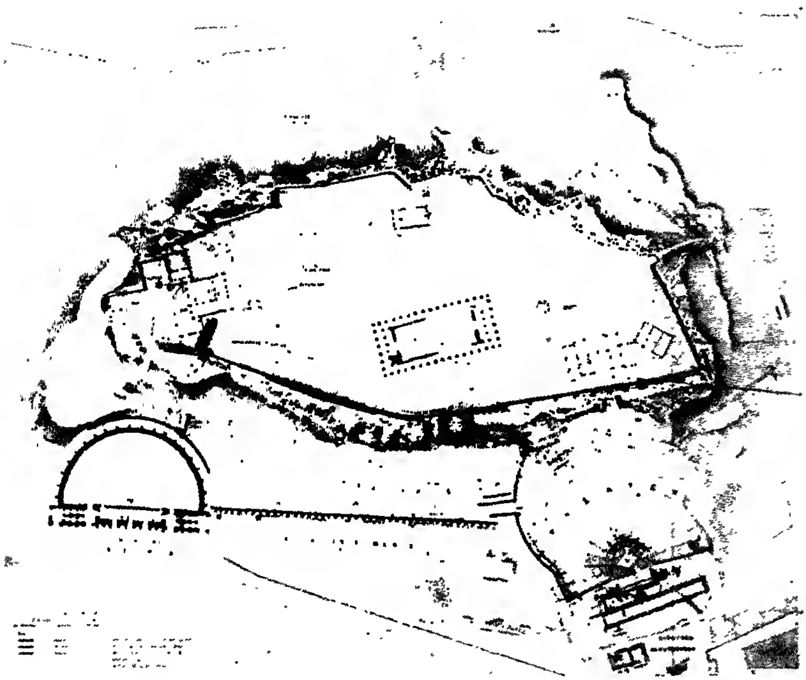

FIG. 9. Plan of Acropolis with axis of Nike temple.

the Mycenaean wall; the highest of these beveled stones was 9.82 m (32 ft $2\frac{1}{2}$ in) above the level of the pavement surrounding the Nike temple.[2] In this case, therefore, our astronomical deductions are plainly erroneous.

[1] In spite of this impenetrable obstacle, the line of the axis of the temple was gravely prolonged to the horizon by Penrose and Nissen, and the date deduced accordingly.

[2] *I.e.* 0.62 m (Nike pavement to Propylaea floor) + 8.317 m (orthostate + 14 regular wall courses) + 0.582 m (special top course) + 0.298 m (cornice) = 9.817 m.

The fact that we know so many of the elements in connection with the Nike temple suggests that we employ the algebraic equation ($X = Ar + R + C + As$) in another manner, in order to restore the astronomical conditions under which the observation of the axis was presumably made. For in this case we know the unknown quantity (X) almost exactly: it was either 427 or 426 B.C., as determined by the archæological evidence (Ar); the religious evidence (R) favors connection with the Panathenaic festival on Hekatombaion 27/28; and the calendar (C) shows that this date would fall on August 16 in 427, but on August 5 in 426 B.C. Now sunrise on August 16, 427 B.C. (the solstice date being June 28), would theoretically have been 21° 32′ north of east (*i.e.* one day earlier than in Table XI, which is calculated for a solstice on June 29); but on August 5, 426 B.C., it would have been 25° 22′ north of east. In the former instance the sun's center would have crossed the line of the axis, 5° 26′ south of east, and so 26° 58′ from the sunrise point, only after it had attained a considerable altitude above the theoretical horizon. On account of the high altitude angles it is preferable to use the data accurately worked out for navigation tables.[1] Thus, with a latitude of 37° 58′, an azimuth of 95° 26′ from true north, and a sunrise declination of 16° 13′ on August 16, 427 B.C.,[2] the true altitude to the center of the sun's disk would be 33° 42′.[3] Similarly, with a sunrise declination of 19° 15′ on August 5, 426 B.C.,[4] the true altitude of the center

[1] Cf. *The Sumner Line of Position* (Washington, 1924), p. 291.

[2] Table XI gives the sunrise declination as 16° 31½′ for August 16; but this result, being calculated for a solstice on June 29 instead of June 28, must be diminished by one day (0° 18½′).

[3] The navigation table (*Sumner Line*, p. 291) gives, for latitude 38°, declination 16°, and azimuth 95.2° or 96.0°, true altitudes of 33° or 34° respectively. Double interpolation is necessary: the additional 0° 13′ of declination requires reductions of 0.312° or 0.325° respectively in azimuth, giving azimuths of 94.888° or 95.675° for true altitudes of 33° or 34° respectively; the difference in azimuth being 0.787° for 1° of altitude, the observed azimuth of 95° 26′ = 95.433° requires an altitude 0° 42′ more than 33°, or 0° 18′ less than 34°, that is, 33° 42′.

[4] Table XI gives the sunrise declination as 19° 15′ on August 6, calculated for a solstice on June 29; hence, with the actual solstice on June 28, the same declination would hold for August 5.

of the sun's disk would be 39° 2′.[1] At these high angles the refraction is barely more than 0° 1′, so that the apparent center of the sun would be at an altitude of 33° 43′ in 427 B.C., of 39° 3′ in 426 B.C., in the line of the axis of the temple. The apparent top of the sun would then be at altitudes of 33° 59′ or 39° 19′ respectively, and, allowing for the visibility of only 0° 2′ of the sun, the altitude of the obstacle would be 33° 57′ or 39° 17′ respectively. This obstacle, of course, was the Mycenaean wall, which rose higher than the chamfered corner of the Propylaea, 9.82 m (32 ft 2½ in) above the pavement of the bastion. How much higher can only be estimated by placing ourselves in the position of the observer of 427 or 426 B.C.

The question of the precise position of the observer is ordinarily of comparative unimportance because of the great distance of the horizon. But in the case of the Nike temple, with the Mycenaean wall rising only a few meters away, exact definition is necessary. Fortunately the position can be defined fairly closely, in view of the recent discovery, by Balanos, of the older shrine below the present temple.[2] No details are yet published, but from the preliminary statements, and from my own study of the levels many years ago, it is possible to imagine that the situation just before the erection of the new temple was approximately as represented in Fig. 10. In making the observation, one must have stood on the lower level of the older shrine, and presumably behind it so as to be under the middle of the present temple. In Fig. 10 the observer is placed as far back as possible, near the west face of the older bastion, and so not more than about 5.50 m behind the east edge of the stylobate of the present temple, which in

[1] The same navigation table (*Sumner Line*, p. 291) gives, for latitude 38°, declination 19°, and azimuth 95.8° or 96.7°, true altitudes of 39° or 40° respectively. The additional 0° 15′ of declination requires reductions of 0.390° in azimuth, giving azimuths of 95.410° or 96.310° for true altitudes of 39° or 40° respectively; the difference in azimuth being 0.900° for 1° of altitude, the observed azimuth of 95° 26′ = 95.433° requires an altitude of 0° 2′ more than 39°, or 0° 58′ less than 40°, that is, 39° 2′.

[2] Cf. *A. J. A.*, 1936, p. 145; *Jahrb. deut. arch. Inst.*, 1936, *A.A.*, 99. Mr. Balanos kindly showed me the remains of the older shrine during my visit in 1937.

turn (at its central point) is 16.20 m from the foot of the Mycenaean wall; thus the observer's eye would be about 21.70 m (71 ft 2 in) from the wall. Since the level of the older bastion was about 1.30 m below that of the present marble pavement, the eye of the observer standing on the older level would have been about 0.32 m above the new pavement level.

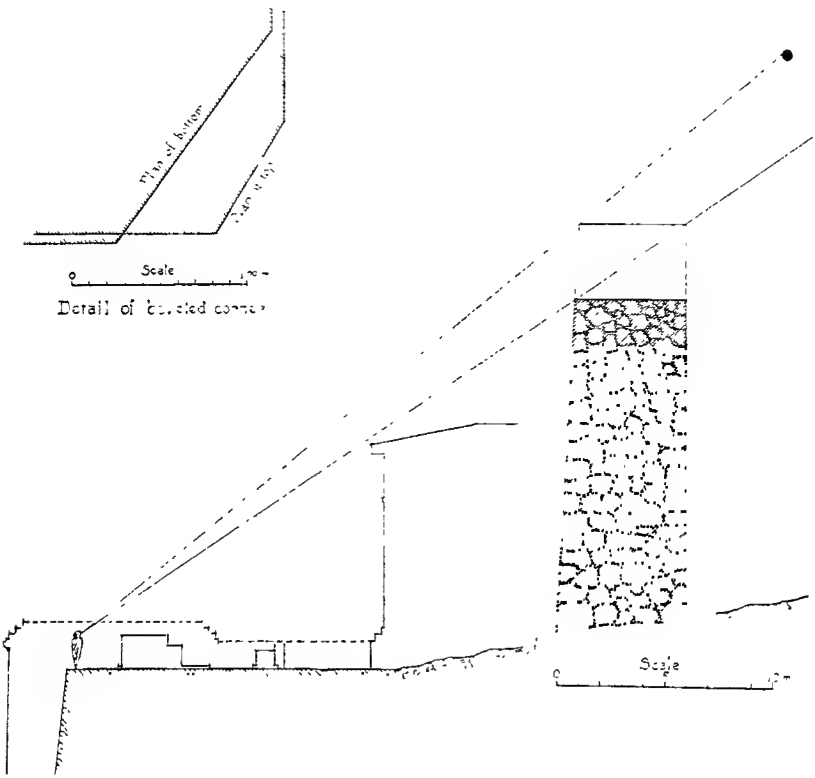

FIG. 10. Section of Nike precinct and Mycenaean wall.

At a distance of 21.70 m and an altitude of 33° 57′, the height of the wall would be 14.61 m above the eye, and so about 14.93 m above the present pavement. At an altitude of 39° 17′, the height of the wall would be 17.75 m above the eye, and so about 18.07 m above the pavement. These heights are subject to modification, however, because of the inward batter of the wall. The amount or rate of this batter

can still be measured, since the vanished profile of the wall is preserved on the chamfered corner of the Propylaea (Fig. 10), showing that it leaned backward about 0.41 m in a height of 6.885 m,[1] an angle of 3° 24′ from the vertical. Since the bottom of the Mycenaean wall, resting on solid rock, lies about 0.40 m above the present pavement of the bastion (and so about 0.08 m above the observer's eye), the top would have battered inward 0.86 m or 1.05 m respectively at heights of 14.53 m or 17.67 m above the eye. In consequence, the ray of vision would actually intersect the battering wall plane at levels 0.60 m or 0.90 m higher (Fig. 10), and so about 15.53 m or 18.97 m above the present pavement, 16.83 m or 20.27 m above the older level.

The observer might, however, have been standing a little closer to the rear wall of the old shrine than he is represented in Fig. 10, reducing the distance to the Mycenaean wall by about 2.00 m. In fact, such a position might have been the more convenient, since the direction observed with reference to a mark or stake on the top of the Mycenaean wall could have been more easily recorded on the top of the demolished rear wall of the shrine. Such a position, about 19.70 m (64 ft 8 in) from the foot of the Mycenaean wall, would have brought the top of the wall (if vertical) about 13.26 m above the eye (13.58 m above the present pavement) in 427 B.C., or 16.11 m above the eye (16.43 m above the present pavement) in 426 B.C. The inward batter, in heights of 13.18 m or 16.03 m above the foot of the wall, would have been 0.78 m or 0.95 m respectively. And the additional height due to the inward batter would have been 0.54 m or 0.81 m respectively, making the total 14.12 m or 17.24 m above the present pavement, 15.44 m or 17.56 m above the older level.

[1] The chamfer removed a triangle which measures, at the top of the dado (orthostate) course, 1.20 m and 0.88 m on the two sides at right angles, or 0.71 m in altitude measured normal to the chamfered base. At the top of the fourteenth course (6.885 m) above, the triangle measures 0.64 m and 0.38 m on the two sides at right angles, or 0.32 m in altitude measured normal to the chamfered base. The difference of 0.39 m in altitude is further increased by the fact that the south wall leans inward (northward) by 0.050 m in fourteen courses (6.885 m), and the east wall leans outward (eastward) by 0.070 m in the same height. Thus the average inward batter of the Mycenaean wall was 0.41 m in this height.

We may now compare these four estimates of the height of the Mycenaean wall (14.12 m, 15.53 m, 17.24 m, and 18.97 m above the present pavement) with the situation in the time of Pericles. Then the main building of the Propylaea, and particularly the eaves of the east portico which cut through the Mycenaean wall instead of abutting against it, rose to a level higher than the chamfered corner of the Southwest Wing by 3.80 m.[1] The apex of the east pediment, the topmost part of the building, was 2.70 m higher, and so attained a level 15.70 m (51 ft 6 in.) above the floor of the west portico, or 16.32 m (53 ft 6 in.) above the pavement of the bastion. It is hardly probable that the Mycenaean wall would have been permitted to rise as high as the apex of the east pediment of the Propylaea, or even as high as the apex of the west pediment which was 1.47 m lower and so 14.85 m (48 ft 9 in.) above the bastion. The most conservative of these estimated heights, therefore, about 14.12 m (46 ft 4 in) above the present pavement of the bastion, or 13.72 m (45 ft) above the foot of the wall itself, satisfactorily fits the conditions in the time of Pericles, after the construction of the Propylaea, and before the erection of the temple of Athena Nike.[2] This means that the little temple was begun, not in 426, but on August 16, 427 B.C., a more suitable date from all points of view because it allows more than three years, rather than two, for its execution.

The Olympieum should be associated, as we have seen, either with the Diasia on Anthesterion 22/23 or with the Olympieia on Mounichion 19, the Julian dates being April 3

[1] *I.e.* 1.449 m (difference in floor levels of the west and east porticoes) + 8.530 m (column height) + 3.025 m (entablature height, including sima) = 13.004 m as compared with 9.20 m for the height of the chamfered corner above the floor level of the Southwest Wing.

[2] Before the time of Pericles we may assume that the Mycenaean wall was more than 13.72 m (45 ft) high. A greater height would most satisfactorily account for the more southerly orientation of the older shrine; it may be possible to estimate this greater height after the particulars of the location of the shrine are made known. We may assume that Mnesicles, the architect of the Propylaea, failing in his effort to have the Mycenaean wall entirely removed, at least obtained permission to trim off its upper part to be more in conformity with the design for the Propylaea.

or September 24, within the years 530–510 B.C. It is obvious that neither festival could be associated with the autumn date. With the spring dates, the new year day *following* Anth. 22/23 = April 3 would have been August 7,[1] and that following Moun. 19 = April 3 would have been June 12/13.[2] Glancing at Table VI, we see that even the latest possible new year days within this period, August 1 in 531, August 3 in 523, August 5 in 515 (IV), or August 2 in 512 B.C. (VII), would yield discrepancies of six, four, two, or five days, all too great to permit consideration of the Diasia, with the possible exception of that in 515 B.C., which is probably too late. On the other hand, with the Olympieia, the suitable days would be June 13 in 532 (III), June 10 in 529 (VI), June 15 in 524 (III), June 12 in 521 (VI), or June 14 in 513 (VI). The first of these is undoubtedly too early; and the last seems improbable in that it would so closely follow the assassination of Hipparchus at the Panathenaia of 514 B.C.; Hippias was quite unlikely to have undertaken large public works at the very moment of his struggle with the Alcmaeonidae. Of the two other dates, the later (521) is to be preferred because seven years rather than ten seem more proportionate to the amount of work accomplished (assuming that it stopped in 514). Thus we adopt April 3, 521 B.C., as the date of the older temple.

The "Theseum" is probably to be associated with the Chalkeia on Pyanopsion 29/30, the Julian date being March 10 or October 18, within the period 455–435 B.C. Eliminating the spring date as inappropriate, it is evident that the new year day *preceding* Pyan. 29/39 = Oct. 18 would have been June 23.[3] According to Table VI, the suitable days would be June 22 in 457, June 23 in 449, or June 24 in 441 B.C. (VI). Of these, the first date seems too early for the archæological evidence, and the last too late. As for the middle date, since the summer solstice in 449 was on June 28 rather than June 29

[1] *I.e.* 8 + 118 = 126 days after April 3.
[2] *I.e.* 11/12 + 59 = 70/71 days after April 3.
[3] *I.e.* 118 − 1 = 117 days before Oct. 18.

(which was taken as the basis of Table VIII), we must adopt October 17, 449 B.C., as the date of the temple (correcting the new year to June 22).

The two temples at Rhamnus should be associated with the Nemesieia which probably were celebrated on Bœdromion 5, the Julian dates being April 1 or September 26 for the older temple and March 27 or October 1 for the later, within the periods 530–490 and 438–413 B.C. respectively. In both cases we discard the inappropriate spring dates. With the autumn date for the older temple, the new year day *preceding* Bœd. 5 = Sept. 26 would have been July 25.[1] Thus the suitable dates would be July 23 in 533 (II), July 25 in 525 (II), July 23 in 522 (V), July 27 in 517 (II), July 25 in 514 (V), July 27 in 506 (V), July 24 in 503 (VIII), or July 25 in 495 (VIII). The possibilities are rather too numerous to permit a decisive choice; yet it is obvious that, if we limit ourselves to a possible error of one day, we reduce the number of available years by half (525, 514, 503, 495), and of these in turn one (525) will be eliminated when we consider the calendar as a whole.[2] Of the remainder, 503 B.C. is one day in error, and 495 B.C. so shortly precedes the battle of Marathon as to seem doubtful. I adopt, therefore, July 25, 514 B.C., which seems very satisfactory because of the resemblance of the workmanship to that of the great retaining wall of the temple at Delphi, being erected at that very time. It offers, furthermore, an interesting coincidence in that it would immediately follow the murder of Hipparchus at the Great Panathenaia of that year; possibly Hippias, who seems to have had a predilection for the region of Marathon, founded it on the occasion of the Genesia festival in honor of his dead brother. As for the later temple, the new year day *preceding* Bœd. 5 = Oct. 1 would have been July 30. If we consult the tables (cf. Table VII) for the first two Metonic cycles (432–394), it is evident that after 432 B.C. the new year never fell exactly on July 30 (at least before 413), though it fell on

[1] *I.e.* 4 + 59 = 63 days before Sept. 26.

[2] See p. 169, eliminating 525, 522, and 517 B.C.

July 27 in 425 and 414 B.C., the next latest being July 24 in 430 and 411 B.C. None of these is capable of reconciliation, and the political conditions of these years would hardly be suitable for the erection of a temple in an outlying part of Attica. If, however, we recede to the years before 432, we find according to Table VI that we could utilize July 29 in 436 (III), or possibly July 28 in 444 (III). The earlier date is not in agreement with the archæological evidence, which indicates that it should be later than the completion of the architecture of the Parthenon in 438 B.C. The later date, however, is most appropriate, not only because it comes after the dedication of the Parthenon, but also because the temple is of such moderate size that it could easily have been erected in the remaining four years 436–432, and its state of incompletion would be explained by the outbreak of the Peloponnesian War. But since the summer solstice in 436 was on June 28 rather than June 29 (the basis for Table XI), we must adopt September 30 as the date of the temple (exactly fitting the new year on July 29).

At Olympia, the temple of Zeus is to be associated with the Olympic festival on Apollonios 14 or Parthenios 14, the Julian date being April 14 or September 13, within the years 472–456 B.C., and presumably with its foundations laid closer to the beginning of that period. It is obvious that we must disregard the spring date. Even with regard to the autumn date, examination of the table of approximate Olympic dates between 480 and 460 B.C.[1] shows that none will fit exactly, though the years in which the event fell on Parthenios 14 would come fairly close, with discrepancies of nine days in 476, eight days in 468, and seven days in 460 B.C. It might conceivably be suggested that the temple was laid out, not on the occasion of an Olympic festival, but on one of the intervening years, presumably on the corresponding day of the year, which might always have been sacred to Zeus. The only full moons occurring reasonably close to the Julian date would be the following: Sept. 14 at 5:40 P.M. in 477; Sept. 11

[1] See above, p. 144.

at 10:15 P.M. in 474; Sept. 16 at 11:55 A.M. in 469; Sept. 13 at 10:00 A.M. in 466 B.C.[1] It is apparent that the conditions would be met most satisfactorily in 466 B.C., and that one of the other years might be made to agree by a slight readjustment. But such an interpretation, while satisfactory enough for a city like Athens, where the laying out of the Older Parthenon need not have been postponed to the Great Panathenaia (any of the Lesser Panathenaia being equally suitable), would hardly fit an isolated sanctuary like Olympia, which burst into activity only once in four years. The temple would undoubtedly have been started with great ceremony at one of the regular Olympic festivals, when a large crowd was present. Thus we have to choose between the three occasions which come closest to filling the conditions, 476 which is undoubtedly too early, 468, and 460 which is almost certainly too late since it would hardly give time for completion before 456 B.C. If we accept 468 B.C., the date now generally adopted on archæological grounds and consequently the most suitable, our problem is to explain the discrepancy of eight days.

If we compare the Athenian calendar of this period, we note that in 468 B.C., the beginning of a cycle having fallen on July 17, 470, the 25th or 26th month would begin on June 24/25 or July 24/25; the beginning of the 27th month of the Athenian cycle would be August 22/24, and the 14th day of that month would be September 4/6. In other words, the discrepancy from the Athenian calendar would be seven to nine days, just as in our preliminary calculation with reference to the moon. At this point we are reminded of another discrepancy of exactly seven days in another Dorian calendar of this period. Plutarch (*Aristeides*, 19) tells us that the Battle of Plataea in 479 B.C. occurred on Bœdromion 4 according to the Athenians but on Panamos 26/27 (the fourth day from the end) according to the Bœotians; "concerning

[1] See Ginzel, *Chron.*, II, pp. 567–568 (transforming the hundredths of days into hours and minutes, counting the days from midnight to midnight (instead of noon to noon), and adding 1 hr 35 m for the difference in longitude between Greenwich and Olympia).

the discrepancy in the day one need not wonder, since even now many states vary as to the beginnings and ends of months."[1] The discrepancy, furthermore, is in the same direction; the Bœotian calendar in 479 B.C. was seven days behind the Athenian, which would naturally be the more correct in view of closer relations with the Ionian astronomers;[2] and the Elean calendar in 468 B.C. would be eight days behind the moon, and seven to nine days behind the Athenian calendar. We are evidently dealing with the same situation, which can only be explained on the assumption that the Dorian calendar cycles were slightly too long. Since Athens was running at this time with cycles of 2923 days, we may assume that at Olympia the cycles were of 2924 days, the Dorian calendar apparently having been worked out on the assumption that the length of the solar year was 365½ days, two years occupying 731 days and the eight-year cycle 2924 days, which would include three lunar years of 354 days, two of 355 days, and three of 384 days. Then, with a discrepancy of seven days in 476 (compare the Bœotian calendar of 479), the discrepancy would have increased to eight days in 468 B.C. (fitting the orientation). We may confidently assume that the axis of the temple of Zeus was laid out at sunrise on September 13, 468 B.C., equivalent to Parthenios 14, Olympiad 78.

The Heraeum at Olympia recedes to such remote antiquity, the end of the eighth century in its earlier form, that we lack detailed evidence as to the arrangement of the calendar and can only assume that it, too, was laid out on Parthenios 14. The exact date, however, remains as indefinite as ever.

[1] This careful statement seems, therefore, more trustworthy than Plutarch's other allusions (*Camillus*, 19; *Glor. Ath.*, 7) to the date as Bœd. 3, implying a discrepancy of six days.

[2] How far the Bœotian Doric calendar could go astray is illustrated by a decree of the second century B.C. (*IG.*, VII, 517), dated on the first day of one month by the civil year, but by the 16th day of the next month according to the astronomical year. Even granting that the civil year had been delayed by an extra intercalation of a month, as sometimes happened, we must also admit a discrepancy of 15 days, the full moon instead of the new moon appearing at the beginning of the civil month (*cf. Archons*, p. 416, n. 2).

CONCLUSIONS

Before any final evaluation of the results, it is desirable to ascertain whether the evidence thus accumulated is inconsistent at any point, and particularly whether all the orientation dates are in accord with the requirements of the Athenian calendar. For this purpose the orientation dates of the eight Attic temples herein considered are recapitulated in chronological order:

529,	September 7,	Peisistratid temple.	Preceding new year = July 9
521,	April 3,	Olympieum.	Following new year = June 12
514,	September 26,	Old temple, Rhamnus.	Preceding new year = July 25
488,	August 31,	Older Parthenon.	Preceding new year = August 5
478,	September 15,	Erechtheum shrine.	Preceding new year = July 16
449,	October 17,	"Theseum."	Preceding new year = June 22
436,	September 30,	New temple, Rhamnus.	Preceding new year = July 29
427,	August 16,	Nike temple.	Preceding new year = July 20

Thus we have, scattered through an epoch for which exact documentary evidence is lacking, a series of astronomical observations which, when interpreted, should serve as statements almost as explicit as the records of eclipses, for instance, mentioned in ancient literature.

As a preliminary step toward a partial reconstruction of the calendar, we may interpolate these orientation dates in a table (*cf.* Table IV) of the initial days of the eight-year cycles, as shown in Table XIII on page 169.

Analysis of Table XIII brings out several facts with regard to the distribution of the years within the cycles. Thus in the sixth cycle (526–518) the location of a new year day on June 12, 521 B.C., shows that the cycle must be of type *J*,[1] thus determining the quality and positions of all eight years in the cycle.[2] In the seventh cycle (518–510), with a new year on July 25, 514 B.C., the cycle might be any of types *A–E*, which have in common only the fact that the last year

[1] See p. 137.

[2] Incidentally this cycle would eliminate three of the astronomically possible dates for the older temple at Rhamnus: July 25, 525, and July 23, 522, being one month too late for the new year days, while July 27, 517, would force 518/7 to be an intercalary year, directly following the intercalary year 519/8 B.C.

TABLE XIII

ATHENIAN EIGHT-YEAR CYCLES, 566–432 B.C.

B.C.	Hek. 1	Number of Days to Next Hek. 1 cited	Events and Orientations
566	June 30	2923	Institution of Panathenaic cycles
558	July 1	2923	
550	July 2	2923	
542	July 3	2924	Cycle lengthened by one day
534	July 5	1831	
*529	July 9	1093	*Axis of Peisistratid temple, September 7*
526	July 7	1802	
*521	June 12	1122	*Axis of Olympieum, April 3*
518	July 9	1477	
514	July 25	1447	Murder of Hipparchus, August 21
*			*Axis of old temple, Rhamnus, September 26*
510	July 11	2924	
502	July 13	2923	Cycle shortened by one day (Cleisthenes)
494	July 14	1476	
490	July 29	738	Battle of Marathon, October 11
*488	Aug. 5	709	*Axis of Older Parthenon, August 31*
486	July 15	2184	
480	July 7	384	Battle of Salamis, September 27
†			Solar eclipse, October 2
479	July 26	355	Battle of Plataea, September 26
*478	July 16	2923	*Axis of Erechtheum shrine, September 15*
470	July 17	2923	
462	July 18	2923	
454	July 19	1800	
*449	June 22	1123	*Axis of "Theseum," October 17*
446	July 20	2923	
438	July 21	739	
*436	July 29	1447	*Axis of new temple, Rhamnus, September 30*
†432	July 15	1831	Astronomical era of Meton, June 27
*427	July 20		*Axis of Nike temple, August 16*

* = calculated orientation dates.
† = recorded astronomical phenomena.

(511/0) must be ordinary. Half of the tenth cycle (494–486) and all of the eleventh (486–478) have been worked out on the basis of the axis of the Older Parthenon and the eclipse after the battle of Salamis;[1] the tenth cycle was of types *A–B*

[1] *A. J. A.*, 1934, p. 445.

or *D*, the eleventh of type *C*. Nothing more is definite until we come to the fifteenth cycle (454–446), where the location of a new year on June 22, 449 B.C., shows that the cycle must be of type *J*, again fixing the quality and positions of all eight years.

In the seventeenth cycle (438–432) we have a special problem because of the transition to the calendar of Meton in 432 B.C. The orientation of the temple at Rhamnus would indicate that the third year (436/5) began with the twenty-sixth month, so that one of the two first years must have been intercalary, thus fitting any of the types *A–G*. There being, furthermore, only 74 lunar months in these six years, we may exclude types *A*, *B*, and *D*, thus leaving only *C*, *E*, *F*, and *G*. Again, it seems to have escaped previous attention that the calendar quality of the fifth year (434/3) is indicated by the Parthenon expense accounts of that year (*IG.*², I, 352), giving 1800 drachmai in monthly salaries to the supervisors of the Parthenon; this sum is not divisible by thirteen, but was obviously the product of 12 × 150, showing that this year was ordinary, and so excluding types *F* and *G*. Being thus limited to types *C* and *E*, it is evident that, while the calendar quality of the two first years remains uncertain, the remaining four would be O (436/5), I (435/4), O (434/3), and O (433/2).[1] The calendar quality of the sixth year (433/2) has hitherto been made to depend on synchronisms with the Amphictyonic accounts of Delos (*IG.*², I, 377), which might be restored with equal facility to show that this year was ordinary at Athens,[2] or that it was intercalary.[3] The evidence from the temple at Rhamnus, showing that 433/2 was an ordinary year, sup-

[1] The order 435/4 = I and 434/3 = O had already been proposed, but on insufficient grounds, in *Archons*, pp. 317, n. 1, 504.

[2] West, *A. J. A.*, 1934, pp. 1–9.

[3] Meritt (*Hesperia*, 1936, pp. 378–380) prefers this solution because it is in agreement with his earlier supposition (*Athenian Calendar*, pp. 85–86, 88–89) that 433/2 must have been intercalary, in order that the prytany (solar) senatorial year may be rotated backward before 432 B.C. But the restoration of the text which he proposes is no more cogent than that of West, who drew the opposite inference.

ports my theory that the independent (solar) prytany year did not exist before Meton's era date of June 27, 432 B.C. (see Table VII).[1]

The results thus attained may be tabulated as follows:

TABLE XIV

KNOWN NEW YEAR DATES, 566–432 B.C.

566	June 30		514	July 25		470	July 17	
*	* *		*	* *		*	* *	
558	July 1		511	July 22	(O, 354)	462	July 18	
*	* *		510	July 11		*	* *	
550	July 2		*	* *		454	July 19	(O, 354)
*	* *		502	July 13		453	July 7	(O, 354)
542	July 3		*	* *		452	June 26	(I, 384)
*	* *		494	July 14		451	July 15	(O, 354)
534	July 5		*	* *		450	July 4	(O, 354)
*	* *		490	July 29	(O, 354)	449	June 22	(I, 384)
529	July 9		489	July 17	(I, 384)	448	July 11	(O, 355)
*	* *		488	Aug. 5	(O, 354)	447	July 1	(I, 384)
526	July 7	(O, 355)	487	July 25	(O, 355)	446	July 20	
525	June 26	(O, 354)	486	July 15	(I, 384)	*	* *	
524	June 15	(I, 384)	485	Aug. 2	(O, 354)	438	July 21	
523	July 4	(O, 354)	484	July 22	(O, 354)	*	* *	
522	June 23	(O, 355)	483	July 11	(I, 384)	436	July 29	(O, 354)
521	June 12	(I, 384)	482	July 30	(O, 354)	435	July 18	(I, 384)
520	July 1	(O, 354)	481	July 18	(O, 354)	434	Aug. 6	(O, 354)
519	June 20	(I, 384)	480	July 7	(I, 384)	433	July 25	(O, 355)
518	July 9		479	July 26	(O, 355)	*	* *	* *
*	* *		478	July 16		432	July 15	(O, 355)
			*	* *				

In the case of the Elean calendar, likewise, we have a definite basis in the orientation date for the temple of Zeus at Olympia, September 13, 468 B.C. As we have noted, this date is the result of an increasing error which seems to have been identical with that which threw the Bœotian calendar

[1] Upon his identification of the exact form of the independent prytany year, Meritt argued that it had been created by Cleisthenes and that it was in continuous operation from 506 to 404 B.C., more than a century (*Athenian Calendar*, pp. 71–72, 84–126). The opposite extreme is represented by my conclusion that this "solar" year was created at the time of Meton's astronomical reform and that it was in continuous operation for only one fifth of this period, 432 to 411 B.C. (*Archons*, pp. 323–348). Meritt subsequently contracted his limits to about 450 and 409 B.C. (*Athenian Financial Documents*, pp. 152–176; *Hesperia*, 1936, pp. 375–380). Kahrstedt (*Untersuchungen zur Magistratur in Athen*, pp. 84, 88) would now recede to 461 B.C. for the date of the introduction. I still believe, however, that all the evidence favors 432 and 411 B.C. as the limits

seven days behind in 479; at Olympia it was eight days behind in 468 B.C. On the assumption that the eight-year cycles were regularly of 2924 days and so fell behind the Athenian calendar by one day in each cycle, we may estimate that the error would have increased to nine days in 460, and to ten days in 452 B.C. Presumably the error would have been corrected before it had grown much larger, by the drastic omission of a few days in the calendar; for this reason the following table is terminated at 460 B.C. On the other hand, rotating the cycles backward, the discrepancy would be six days in 484, five days in 492, four days in 500 B.C. Then, for five cycles, the Elean and Athenian calendars would rotate in parallel cycles of 2924 days; but in 548 the discrepancy would be reduced to three days, in 556 to two days, in 564 to one day, while in 572 B.C. the Dorian calendar would have been running true. We may, therefore, list the dates of the Olympic festivals between 572 and 460 B.C. as follows:

TABLE XV

CALENDAR OF OLYMPIC FESTIVALS, 572–460 B.C.

572, August 18	=	Parthenios 14
568, August 4	=	Apollonios 14
564, August 20	=	Parthenios 14
560, August 6	=	Apollonios 14
556, August 22	=	Parthenios 14
552, August 8	=	Apollonios 14
548, August 24	=	Parthenios 14
544, August 10	=	Apollonios 14
540, August 26	=	Parthenios 14
536, August 12	=	Apollonios 14
532, August 28	=	Parthenios 14
528, August 14	=	Apollonios 14
524, August 30	=	Parthenios 14
520, August 16	=	Apollonios 14
516, September 1	=	Parthenios 14
512, August 18	=	Apollonios 14
508, September 3	=	Parthenios 14
504, August 20	=	Apollonios 14
500, September 5	=	Parthenios 14
496, August 22	=	Apollonios 14
492, September 7	=	Parthenios 14
488, August 24	=	Apollonios 14
484, September 9	=	Parthenios 14
480, August 26	=	Apollonios 14
476, September 11	=	Parthenios 14
472, August 28	=	Apollonios 14
468, September 13	=	Parthenios 14
464, August 30	=	Apollonios 14
460, September 15	=	Parthenios 14

As we have seen, the Olympic festival was always held in the eighth month of the year, whether this were ordinary or intercalary. Consequently we are enabled to count back from this 221st day of the Elean year, and to establish the corresponding new year days as follows:

TABLE XVI

New Year Days (Thosythias 1) Beginning Quadrennial Olympiads, 572–460 b.c.

B.C.	Tho. 1	Olympiad
572,	January 10	(1447 days)
569,	December 27	(1477 days)
564,	January 12	(1447 days)
561,	December 29	(1477 days)
556,	January 14	(1447 days)
553,	December 31	(1477 days)
548,	January 16	(1447 days)
544,	January 2	(1477 days)
540,	January 18	(1447 days)
536,	January 4	(1477 days)
532,	January 20	(1447 days)
528,	January 6	(1477 days)
524,	January 22	(1447 days)
520,	January 8	(1477 days)
516,	January 24	(1447 days)
512,	January 10	(1477 days)
508,	January 26	(1447 days)
504,	January 12	(1477 days)
500,	January 28	(1447 days)
496,	January 14	(1477 days)
492,	January 30	(1447 days)
488,	January 16	(1477 days)
484,	February 1	(1447 days)
480,	January 18	(1477 days)
476,	February 3	(1447 days)
472,	January 20	(1477 days)
468,	February 5	(1447 days)
464,	January 22	(1477 days)
460,	February 7	(1447 days)

Into further ramifications of the problems of the calendars we need not enter. I feel convinced, in view of the manner in which it has been possible to coordinate the available material up to this point, that additional investigation of the orientations of Greek temples, and of their archæological, religious, and calendar relationships, would throw light on many problems pertaining to the development of Greek culture.

POST-NATAL DEVELOPMENT OF THE HUMAN OUTER NOSE

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CONTENTS

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ABSTRACT

The problems are, how are the size and form of the external nose determined; by what road does it attain its eventual proportions, how does it come to be so remarkably similar in both of identical twins. Light on these questions was gained by measurements of fetuses, infants, and children three to twenty years old. The data discussed are both in masses, giving size-age curves, and in individual "longitudinal" series. The data considered consist of five absolute dimensions and eight ratios. The individual curves of nasal height show an adolescent spurt correlated with that of stature, but different dimensions grow at different rates. In brothers the curves are typically parallel, in identical twins superimposed. The growths of nasal depth, width, salient, are discussed in detail. The ratios of nasal proportions often develop in complex fashion. Indeed, in the development of nasal proportions the bones and cartilages of the face seem quite plastic but, nevertheless, work toward a predestined hereditary form.

PREFACE

The development of the outer nose in man has hitherto been studied from various aspects; mostly the aspect of prenatal development, partly the aspect of its variation as a racial character. Average nasal height, width and nasal index have been determined in a very few cases for the various ages of childhood. No data are available concerning the changes in size and relative dimensions of the nose for individual children during a series. This paper is intended to fill this gap.

In gathering material for the paper I have had the opportunity to make repeated measurements of infants and young children at the Normal Child Development Study of Columbia University, located at Babies Hospital, New York, where Dr. Myrtle B. McGraw has afforded every assistance; also at the Brooklyn Orphan Asylum where the children are mostly of Nordic (Protestant) stock, whose parents are for the most part either separated or sometimes dead. The major part of the work, however, has been done at Letchworth Village, New York state, upon children devoid of neurological symptoms and in one series selected for good development of body and mind. To this study the late Dr. Charles S. Little, Superintendent at Letchworth Village, gave great assistance, as did the various directors of clinics and research, Doctors Howard W. Potter, E. W. Martz and E. J. Humphreys.

Some prenatal observations were made both at the Carnegie Institution's Department of Embryology at Baltimore, with the cooperation of its Director, Dr. George L. Streeter; and at the New York City Morgue. For ages between 2 and 7 years some data were secured from children of the Brooklyn Home for Children, with the cooperation of the late Miss Anita J. Fernandez, Superintendent. My friend, Dr. Morris Steggerda, has contributed data on nasal dimensions of Dutch, Negro and Indian (Navajo and Maya) children; also photographs for Pl. XVII. All assistance thus received is herewith gratefully acknowledged.

Thanks are due to a considerable number of assistants who acted as recorders at one time and another. Especially mention must be made of the assistance of the statistical staff headed by Miss Catherine Carley. To my previous assistant, Mr. William Drager, I am indebted for a vast amount of statistical work and many suggestions. Mr. Wilfred D. Hallock has aided in many ways.

A. STATEMENT OF PROBLEMS

I. Structure of Outer Nose

The human external nose is a striking feature of man. Elongated snouts are possessed by the tapir and elephant, but in these the nostrils are carried to the apex of the proboscis. The same is true of the proboscis monkey, Nasalis lavatus (Wen, '30, Fig. 6) the elongated nose of which otherwise somewhat resembles man's. The anthropoid apes have an external nose that projects very little above the contour of the face (Pls. I, II).[1]

The form of the external nose is determined by the bony and cartilaginous skeleton covered by muscles, connective tissue and skin with its blood vessels and glands (Pl. III, 4–6). Externally are recognized the root of the nose, between the eyes, from which starts the rounded ridge or dorsum of the nose the crest of which is called the bridge. At about the level of the apex of the nose is the basal shelf perforated by the two nostrils (Pl. III, 6, *g*) between which is the nasal septum (*f*). The septum of the nose passes over into the upper lip either at an angle or by a curve of smaller or greater radius (Pl. III, 2, 3). The sides of the nose slope back to an ill-defined margin, where they join the cheek (Pl. III, 1). Below, the sides are expanded on the right and left to form the convexly rounded "wings," the alæ nasi, each of which surrounds a nostril. The wings are separated from the cheek and the upper lip by a sulcus, extending behind and below respectively. The wing is bounded above by the wing furrow and below it forms the lateral wall of the nostril (anterior naris) (Pl. III, 2, 6).

The upper third of the nose ridge, or "nose bridge," is supported by the paired nasal bones (Pl. III, 4, *a*). These are sutured to the frontal bone below the glabella, or hairless patch between the eyebrows. The sagittal contour of the nasal bones is variable, but it is usually concave in the

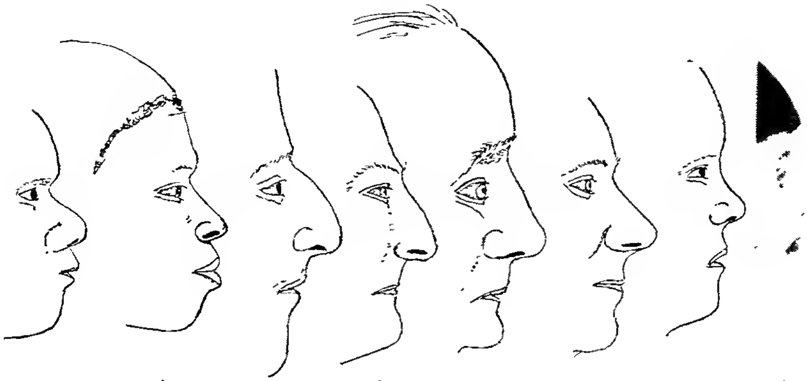
[1] Schultz, '33, p. 252, figures a young gibbon with an unusually prominent nose.

upper part, straight or slightly convex below. The intersection of the suture with the sagittal plane is a point called nasion (Pl. III, 4, 5, *n*). At the lower limit the nasal bones are attached to the paired lateral or "roof" nasal cartilages (Pl. III, 4, 5, *c*) which support the middle third of the nasal ridge and rest, in the sagittal plane, upon the cartilaginous septum of the nose (Pl. III, 5, 6, *f*). Distal to the lateral nasal cartilages and constituting the lower third of the nose lies the pair of great alar cartilages (Pl. III, 4–6, *d*), each of which is a half-ring or horseshoe shaped object supporting the distal portion of the wing of the nose and passing in front of the nostrils from septum (at *d″*) to lateral margin (at *d′*) of the nose. While the median crura (*d″*) of right and left alar cartilages are usually closely applied in man, this is not always true in the lower Primates (Wen, '30, p. 114). In some human individuals there is a deep sulcus between them giving a bifid tip to the nose as described by Lehmann-Nitsche, 1915. Such, then, are the main skeletal elements that determine the form of the external nose: nasal bones, lateral cartilages, nasal septal cartilage, alar cartilages.

The superficial muscles of the external nose are the *procerus* lying over the nasale, by which wrinkles may be made at the root of the nose; and the transverse part of the *nasalis* muscle which helps draw up the lateral margin of the wings of the nose as in snarling (Pl. II, Fig. 2). The alar part of the *nasalis* tends to contract the nostrils. The dilators of the nostrils run from the outer margin of the nostril upward under the transverse part of the nasalis. While in a child who is emotionally undisturbed the muscles of the nose do not prevent comparable measurements of nose form to be made in successive weeks, in disturbed babies these muscles are often contracted and cause relatively great deformation.

The human nose has undergone great mutation in form in the various races of mankind. These mutations reach their highest manifestation in the adult nose. Fig. A shows

some of these forms. *a*. The Australian "black fellow's" nose is a baby-like nose with well developed alæ, but low bridge and short ridge cartilage. *b*. The negro nose be-



a b c d e f g h

FIG. 4. Profiles of face of various races as described in text, being camera drawings of photographs from various sources; photograph of mongoloid dwarf.

sides its great breadth has a low ridge on its distal third and a rather low root. *c*. The Syrian nose has prominent nasal bones and large ridge cartilages. *d–g*. Most northern European noses have a fairly high root and large wings. The roof cartilage may be straight or sharply convex. In central Europe are examples of a concave ridge cartilage. In the Malay archipelago and among certain endocrine types (*h*) there appears a shallow nose approaching that of the gorilla or gibbon.

Seen in full face (Pl. IV) the nose has undergone mutations in height, breadth, nasal index; also in form of apex of nose and visibility of the nostrils from in front. The boundaries of the nose on right and left may be nearly parallel, they may nearly form a triangle, or the form may be intermediate.

In general, members of the best established European sub-races, with least intermixture, approach each other in form. Members of the same family, with many genes in

common, though they may vary as eye color does, often have certain family features of nose form in common.

Finally, in identical twins where the genes are the same the nose form is identical (Pl. V).

Going back, now, to our enumeration of the elements involved in nose form the conclusion seems inescapable that nose form depends first of all on the kind of genes at work on the nasal bones and the cartilages.

II. Hypothesis

The form of the adult nose is, of course, determined by the course of development of the nose and especially of the different elements of nose structure.

From this point of view it becomes necessary to follow the course of development of the individual nose from birth to maturity. It seems to be true that noses at birth are more alike than adult noses; although this is difficult to test statistically because of the fact that the infantile nose changes in form very rapidly; corresponding stages of nose development do not always occur at precisely the same number of days after birth, and the measurements of the dimensions of the baby's nose have a very large error, so that too much dependence cannot be placed on them.

III. Review of Early Ontogeny of the Outer Nose

The first indication of the nose is found in an embryo of perhaps 3 weeks from conception (Keibel-Elze, '08, p. 96, No. 11, of Ivar Broman). The nasal pits are not yet formed; but the lateral epidermis shows signs of thickening though it is not sharply separated from the rest of the ectoderm. This is at the stage of 30 primitive segments.

In Keibel-Elze's No. 13 (of Tellyesnicky), 4 mm long with 34 somite pairs, there is a paired clearly convex nasal field. In an embryo (No. 20) of 5 mm and 38 somites (Vestberg), the nasal fields are still convex and are about 0.45 mm to 0.36 mm diameter. In a 6.75 mm embryo (No.

21) the field is flattened. In an embryo (No. 24, of Keibel-Elze) of 6.5 neck-rump length, about 21 days old, the nasal field is slightly depressed so that a nasal pit is beginning to form. The nasal pits are still lateral and there is less than a millimeter distance between the eye and nasal pit on each side (Pl. VI, 1). At 11 mm, or 30 days, the nasal pit has moved forward (by the forward growth of the maxilla) from the eye accompanied by the enlargement of the lateral nasal processes, the progenitors of the nose alæ. The pits that are thus approaching each other are sinking deep below the surface (Pl. VI, 2). Between the pits a slight swelling appears, having a median notch at the vertex margin of the prostomion. Thus the outer nose with its nostrils and alæ is foreshadowed.

The question is raised whether the movement of the nostrils from the eyes and toward each other is absolute or only relative to the enlarging head. To get the answer I have measured the distance between the innermost margins of the nostrils and also between the lateral margin of the head in Figs. 346 and 350 of Kollmann. In the younger embryo the distance between nostrils is 22 mm on the drawing or 66 per cent of transverse head distance; in the older, 18 mm on the drawing or 30 per cent of the head width. As the drawings are nearly to the same scale, we may conclude that the nostrils not only approach relatively in marked degree, but also, to a much less degree, absolutely. The nostrils are, as it were, pushed together toward the midfrontal line.

The nostrils having once attained a distance apart of 0.5 mm proceed to separate slowly with the growth of the face but relatively to face breadth, continue to approach each other (Pl. VI, 3).

Later fetal stages of nose development are shown on Pl. VIa.

B. MATERIAL AND METHODS

The institutions from which the children came are listed in the Preface. The babies, at Babies Hospital, and the

children from Homes and Asylums are of normal intelligence and development. Those of school age attend public schools into the High School and are well cared for in the institutions. As stated, the great majority are not orphans, but the victims of homes broken up owing to separation of parents. Formerly orphan asylums contained defective children; but not now. Letchworth Village, on the other hand, is an institution for mental defectives. But such defectives range, on the one hand, from only slightly backward children, sometimes committed, because the family has to be broken up, to ensure the safety of the children, to children who, on the other hand, have to be spoon fed, and are permanently and severely crippled mentally and physically. Some years ago about 100 of the middle to lower grade children who were without marked physical or neurological defect were measured, the "I" series; later about 100 middle to high grade children were measured in a study undertaken jointly with Miss Blanche M. Minogue and published in 1930. This is known as the LVD_I series. Some of these boys were followed individually for 12 years or more. Finally, for the past 5 or 6 years we have been making observations biannually, or more frequently, upon 120 boys and girls of the highest grade. In general, most children at Letchworth Village are constitutionally dwarfed both physically and mentally. But the last LVD_{II} series were mostly of the type that is given parole or discharged after training and had nearly a standard development.

The general method has been to measure the dimensions of the outer nose of the same child through as many years as possible. The babies were measured at first fortnightly, later monthly. While about 400 children have thus been followed probably not more than 30 have been measured for as many as ten years.

The nasal dimensions measured were height, width, depth, salient and depth of root-bridge. Various ratios have been computed from these data.

As the work progressed it became refined so as to secure

a checking of measurements. A device used during the last two years has been described in Davenport, 1937 and 1937a.

C. RESULTS

In considering the results of the measurements on growth of nose it will be convenient to consider in order the growth of the single dimensions, both by mass statistics and by individuals. Later will be considered the nasal ratios and the ratios of nose dimensions to other dimensions of the face.

I. Absolute Dimensions

1. Growth of Nasal Height

a. Definition and Methods.

Nasal height is the distance between the nasion and the subnasale. The position of the nasion is hard to locate in the living, since the suture between frontal and nasal bones is not a prominent landmark. In infants it is frequently marked by a transverse wrinkle or crease where, apparently, the nasal bone, quite ununited to the frontal, may move upon it as at a hinge joint. In older children the suture can usually be felt as a groove, lying at or usually a few millimeters above the root of the nose at its lowest point. In adults the suture is sometimes nearly obliterated and nasion must be found indirectly.[1]

[1] Many investigators have discussed the location of the nasion. Martin ('28, pp. 145–147) says: "derjenige Punkt der Nasenwurzel, der von der Mediansagittal-Ebene geschnitten wird. Die Nasenwurzel entspricht nicht der am tiefsten Stelle des Nasenrückens, die meist im Gebiete der Ossa nasalia gelegen ist, sondern der Sutura naso-frontalis, deren Verlauf nach einiger Übung trotz des vorhandenen Nahtgewebes und des meist dünnen M. procerus (M. depressor glabellae nach H. Virchow) auch am Lebenden festgestellt werden kann. Man findet den Punkt am besten, wenn man seine rechte Hand ruhig auf den Kopf des zu messenden Individuums legt und mit dem lateralen Rand der Daumenbeere unter leichtem Druck die Haut auf der Nasenwurzel auf und ab schiebt. Man beachte, dass das Nasion in der Regel im Niveau der medialen Enden der härenen Augenbrauen, meist an deren Unterrand, nicht in der Höhe der Lidspalte gelegen ist."

Many investigators have found it difficult or impossible to locate the nasion in the living by palpation and so other means have been proposed. Oetteking ('31, p. 475) finds the nasion at the midpoint between the horizontal

The lower limit of nose height is the subnasale.[1]

The nose height of the adult varies greatly in individuals of the different races (Fig. A). It also varies in adults of any human race and even in members of one family. Our problem is how does the nose height grow so as to produce the differences in form that we see?

The growth of nose height may be considered absolutely or relatively to the growth of the face as a whole. In the case of fetal and early post-natal development mass statistics will be made use of to a certain extent. From 5 to 19 repeated measurements of the same individuals are available.

b. Mass Statistics.

α. General.—The growth of nasal height accompanies that of the body as a whole. The dimension first appears when an external nose can first be spoken of. This is per-

plane passing through the tangent to the lower bounding curve of the eyebrows (as representing the crest of the supraorbital boundary of the orbit) and the caruncula lachrimalis, at the nasal angle of the lids. This point, however, he adjusts "to the individual physiognomic configuration," since he finds "the general appearance of the living nose" has to be taken into account. This qualification, unfortunately, leaves us nowhere. Quite as useless is Hrdlička's ('20, p. 72) direction for finding the nasion in the majority of subjects. "We must rely upon knowledge of its location derived from extensive observations on skulls and dissecting room material." Fortunately, since we dealt with children mostly under eighteen years of age the nasion could generally be detected by palpation.

[1] The subnasale is defined by Martin ('28, p. 147), as follows: "derjenige Punkt, der an dem einspringenden Winkel der Unterrandes der Nasenscheidewand und der Integumentaloberlippe gelegen ist. Der Punkt ist stets da zu suchen, wo eine an die Nasenscheidewand gelegte Tangente die Oberlippe trifft." This definition may suffice for many cases, especially all negroes (Pl. IV, Figs. 6, 8). But unfortunately there are among persons of European stock those in which the lower margin of the nasal septum and the front of the upper lip form parts of an arc of a circle, of large radius, so that a tangent to the lower margin of the nasal septum does not cut the upper lip anywhere (Pl. III, Fig. 3). The definition of Hrdlička ('20, p. 22) also does not help us, viz: "inferiorly—the nasal septum where it joins the upper lip." Of course the problem in many cases is just where does the nasal septum end and the upper lip begin? When the anterior nasal spine is well developed this may serve as a landmark for subnasale. I think it is only fair to say that in a certain percentage of cases the subnasale is absent and nose height cannot be measured precisely, and such cases are not considered in this report.

haps in the embryo of 18 mm CR length, when it is 7 weeks old. (Pl. VI, Fig. 2; Pl. VIa, Fig. 1). Since the outer nose rises as a sort of rounded papilla, from the beginning about 1 millimeter in diameter, growth of this dimension in length does not properly begin at zero. For just as a balloon that is being inflated does not start at zero dimension, just because the stuff out of which the balloon is to be formed al-

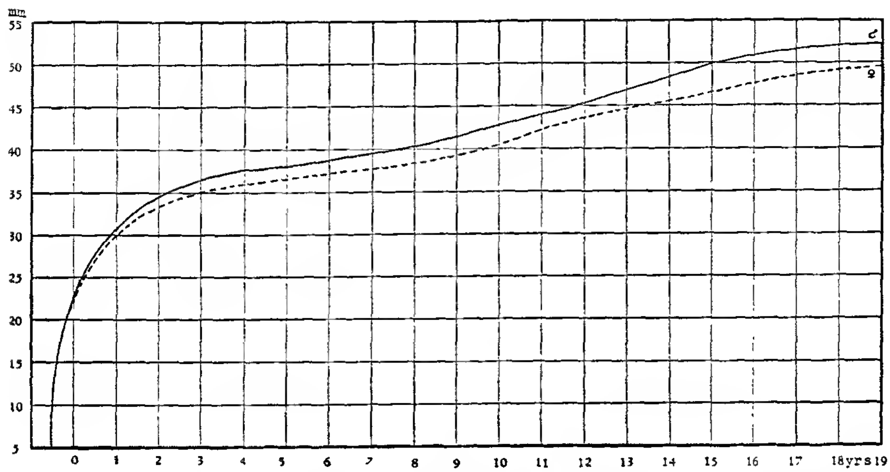


FIG. 1. Mass curves of growth of nasal height, embryo to maturity, combining data of A. H. Schultz (prenatal), Babies Hospital, Brooklyn Home, BOA, and Letchworth Village. Male and female. (N.B. In the strict standard series the male and female curves nearly coincide from birth to 8 years.

ready has a dimension, so the stuff out of which the nose is to develop has a considerable volume before an outer nose can be measured. Of course, strictly, no dimension of the body begins at zero since the youngest and smallest part of the body—the fertilized egg—has already a measurable size.

At 12 weeks after conception the nose is 5 mm high; at the 6th month, 17.5 mm; at birth about 22 mm [1] (Pl. VIA); at maturity around 50 mm. The velocity of growth of this

[1] Houzé ('88) gives mean nasal height of 24 newborn French babies, 21; 25 Bruxellois, 18.2. Blind ('90) finds, for 100 Munich neonates, nasal height, 23 mm.

dimension is relatively rapid from the 12th intrauterine week to the 24th week, averaging 1 mm per week. It shows up a bit before birth; has a slow annual increment at 3 to 6 years of about 0.7 mm per year (Fig. 1).

β. Sexual.—*Male.* Already at birth there is a trace of a sexual difference in this dimension (Fig. 1). In the males the velocity per year becomes progressively reduced up to 7 years, as follows:

Birth to 3 months	11.0 mm per annum
3 to 6 months	9.5 " " "
6 to 9 "	6.4 " " "
9 to 12 "	6.0 " " "
12 to 18 "	4.2 " " "
18 to 24 "	3.0 " " "
2 to 2½ years	2.4 " " "
2½ to 3 "	2.0 " " "
3 to 4 "	1.0 " " "
4 to 5 "	0.7 " " "
5 to 6 "	0.7 " " "
6 to 7 "	0.7 " " "
7 to 8 "	0.6 " " "
8 to 9 "	1.3 " " "
9 to 10 "	1.2 " " "

From 8 to 11 years the speed increases in relation to the juvenile spurt of growth, slows down a bit and then increases to 1.3 mm per annum from 12 to 13 years to 1.4 mm per annum from 13 to 14 years and to 1.6 mm per annum from 14 to 15 years, as a phase of the adolescent spurt of growth. After that the velocity of growth falls off markedly.

Female. From birth on to 2 years the nose height of the female is about the same as that of the male. After two years the velocity of growth of nose in length falls behind that of the male, being only about 0.7 mm per annum from 3 to 5 years and about 0.5 mm p.a. from 5 to 8 years. From 8 years to 10 growth is speeded up to about 1.1 mm p.a. From 10 to 12 it is 1.2 mm p.a. This is the period of beginning of adolescence in the girl. At 16 years the nose height is 2 or 3 mm shorter than that of the boys.

Comparative. There are not many data for a compara-

tive view of sex differences in nasal height. Schwerz ('10) has traced nasal height in both sexes from 6 to 17 years. His measurements of Schaffhauser children are mostly about 3 mm greater than mine; there is possibly a difference in locating the nasion. Nasal height of girls is less than that of boys from 9 to 17 years except that it is the same as of boys from 12 to 14. Hrdlička ('25, p. 248) finds the adult average nasal height of white women in the United States to be 4 mm less than that of men, *e.g.,* 49.4 mm ♀ to 53.5 mm ♂. His absolute average height is thus closely comparable with mine. In Reuter's Hinterpommern (Martin, '28, p. 558) the mean nasal height is greater in the male from 6 to 11 years.

In Martin's ('28, p. 553) table average nasal height is higher in the males in all cases when comparison is possible, viz., South Andamaners, White Russians and Lithuanians; the excess being about 2 to 5 mm or about 10 per cent.

In the case of Navajo Indians, measured by Dr. Steggerda (unpublished data), the nasal height is, at 8 to 10 years, greater in the male; from 11 to 13 it is about 1 mm greater in the female; at 14 to 15 years it is greater in the male again. The data are inadequate for later ages.

In the case of Maya Indians (also unpublished data of Steggerda) the average nasal height of the male is superior except at 7 and 8 years (when it is only slightly greater) and again at 11 to 15 years when the mean female nose height is 2 to 4 mm greater.

In the case of Dutch children of Holland, Michigan (again, unpublished data of Steggerda), the mean nasal height is greater in the boys except at 6 years, but in this series few children were measured older than 12 years.

As for American Negro children of Alabama (Steggerda) the nasal height was superior in the females, except at ages 6 and 11. There are few observations beyond 13 years.

The excess of mean male nasal height of about 10 per

cent is associated with the fact that the morphological head height of the average boy of our LVDII series is not far from 10 per cent greater than in the average girl. Accordingly the proportion of nasal height to face height is about the same in the two sexes. This matter will be discussed more fully in another chapter.

Certain mean values of nasal height in mm as determined by Houzé ('88), Gray and Ayres ('31, pp. 72–147) and our BOA series are given in Table 1.

TABLE 1

MEAN NASAL HEIGHT

Age in Years	Houzé '88	Gray and Ayres '31				Babies+BOA			
		Mean mm	Per Cent ♂ Stat.	Mean mm	Per Cent ♀ Stat.	♂ mm	S E	♀ mm	S.E
New born...	18.2					23.6		23.3	
0.5–1.0.....	25.3					27.0		27.5	
3.5–4.49..	30.7	36.5	3.33						
4 5–5.49.		40.4	3.60	42.9	3.80				
5.5–6.49.		42.0	3.53	43.6	3.64				
6.5–7.49.	34.3	43.1	3.47	44.8	3.61	36.8	1.12	37.5	0.93
7.5–8.49..		44.5	3.40	45.3	3.45	38.2	0.80	38.3	1.25
8.5–9.49...		45.5	3.36	46.3	3.42	41.0	0.98	39.4	1.08
9.5–10.49..	38.2	46.7	3.32	47.0	3.37	41.6	0.94	40.0	1.27
10.5–11.49.		48.1	3.32	48.6	3.34	43.3	0.90	41.9	0.79
11.5–12.49...		48.8	3.26	49.5	3.27	44.3	0.88	42.4	0.65
12.5–13.49..	42.4	49.9	3.20	50.4	3.21	44.8	0.73	43.6	0.61
13.5–14.49..		51.6	3.19	50.7	3.18	46.0	0.90	45.3	0.71
14.5–15.49.		53.0	3.15	51.3	3.16	47.0	0.73	46.6	0.76
15.5–16.49.	45.0	53.6	3.11	51.3	3.16	48.1	0.98	47.4	0.76
16 5–17.49		54.6	3.13	52.2	3.19	49.2	1.10	49.2	1.08
17.5–18.49		55.1	3.14	52.4	3.19				
18 5....	46.0	54.6	3.11	51.4	3.12				

It will be observed that nasal height in Gray and Ayres' series exceeds that in mine by about 10 per cent, and Houzé's by about 20 per cent, no doubt due to a difference in technique. Gray and Ayres state (p. 29) that in finding subnasale they had difficulty; but "endeavored to follow Hrdlička."

In both sexes the mean relative nasal height diminishes with age. In the younger children in whom the legs (an important part of stature) are relatively short the ratio of nose length to stature is large, about 3.7 per cent and in the full grown children it is small, 3.1 per cent. Thus, speaking broadly, one may say that for Nordics in the adults of both sexes the nose height is about 3 per cent of total height (stature). That is, in an adult male 1750 mm tall we may expect, with our technique, a nose about 52.5 mm high; and in an adult female of 1600 mm a nose about 48 mm high.

Thus there is a close relation between growth of the nose in height and that of the body as a whole. It is probable from the averages given above that there is a little lag in the growth of the nose over the legs. Thus, in girls, at 15 years the ratio of nasal height to stature reaches for the time a minimum of 3.16 per cent. In boys the minimum of 3.1 occurs a little later, at 16 years. Later, however, albeit only for two years, the nasal height tends to catch up somewhat with the developing stature. Again, as special computations show, while the mean age of the male spurt in stature is 14.92 years that of spurt in nose height is 15.23. Possibly the growth of the nose is slightly delayed beyond that of stature. However, little stress should be laid upon this difference as the standard error of the mean age of the maxima is about $\pm$ 0.22.

γ. *Social.*—A comparison of our four groups of U. S. whites shows no significant difference in nasal height between BOA males and the LVD or even the Idiot series (Fig. 2). The Mongoloid dwarfs, on the other hand, have strikingly low noses, apparently largely due to slow growth between 7 and 11 years. The velocity of growth increases between 11 and 14 years up to 0.8 mm per year. At 15 years the nasal height is 42.6 mm (or 11 per cent) below that of BOA boys, while their stature is 13 per cent below that of BOA boys. The prevalent view that idiots have

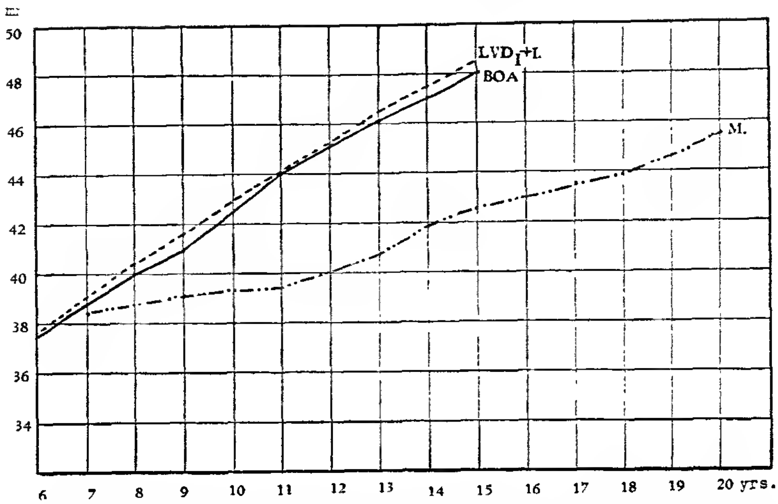


FIG. 2. Mass curves of growth of nasal height in three social groups: Standard, Brooklyn Orphan Asylum (BOA), first Letchworth Village Developmental series (LVD_I) plus Idiot series (I), and mongoloid dwarfs (M). Males.

exceptionally large noses is not generally true, at least for nasal height.

δ. *Racial.*—The nose of the Nordics[1] in our LVD_I series, male, gives a nearly straight line curve of growth in height from 6 to 16 years, as we have already seen. The Mediterraneans have a nose that is about 1 mm higher at 8, rising to 3½ mm higher at 12 years—48 as compared with 44.5 mm (Fig. 3). At the latest available age (15 years) the nasal height of the Mediterraneans is about 49 mm as contrasted with about 48 mm of the Nordics.

The height curve of the "Negro" nose in our LVD_I series lies between that of the Nordic and Mediterranean. It is about 1 mm above the BOA standard. It reaches 48.5

[1] "Nordic" may be defined here, once for all, as indicating, briefly, the origin of family stock from Northern Europe, including British Isles, Scandinavia, Northern Germany. Excluded are: Italians, the few French and Spanish and the Jews. Anyone who has traveled in Europe has noticed that the people living around the North and Baltic Seas are, on the average, different in size from those living around the Mediterranean Sea. Persons of "Old American" stock, though mixed with more recent European immigrants are prevailingly Nordic and are frequently, in the statistics of this paper, so treated.

mm at 14 years. Herskovits' ('27, p. 308) average developmental series runs parallel to ours from 9 to 13 years. At 19 years his nasal height of 52.7 mm much exceeds that of

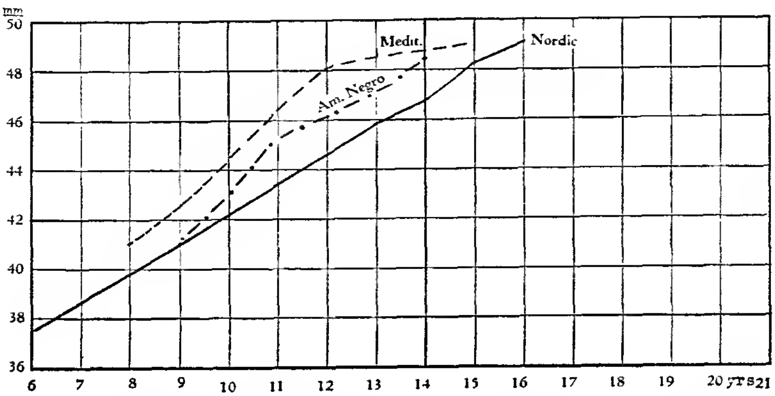


FIG. 3. Mass curves of growth of nasal height in three racial groups: Nordic + U.S., Mediterranean (Medit.), Negro. All from Letchworth Village, $LVD_{I} + LVD_{II}$ groups.

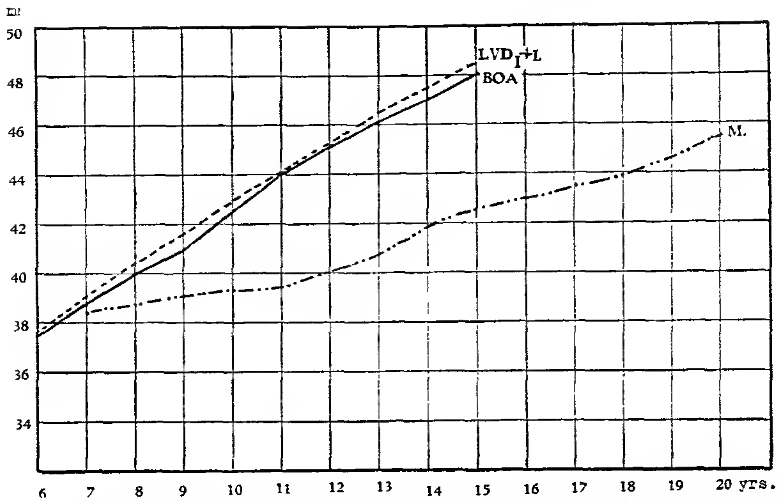
our U. S. series. Weninger ('27, p. 68) gives the mean height of adult noses of West African Negroes as 48.0 mm.

On account of the varied standards adopted by different authors for nasion, a more extensive comparison will not be justified.

c. Individual Data.

Since a comparison of mass and individual growth curves shows that the former tell little about individual growth, and since it is biological phenomena and not statistics that interest us, we must consider certain curves of individual growth, as follows:

a. Comparative.—Figure 4 shows the curves of absolute growth of nasal height of certain boys (separated into two groups to avoid over-crowding of the figures). All curves show a steady increase of nasal height; but the increase is not uniform at all ages. In Nos 3, 5, above and 1, 2, 3, 4, below there are periods of about 2 years' duration during which there is little or no growth. These periods of slow



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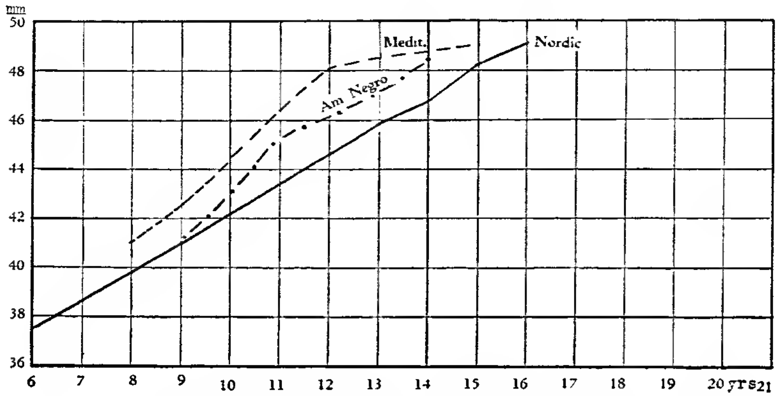

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growth are at different ages in the different individuals. In three cases the still-stand is centered at about 12 years; in two at 11 years; this is the period that follows the juvenile spurt of growth and precedes the adolescent spurt.

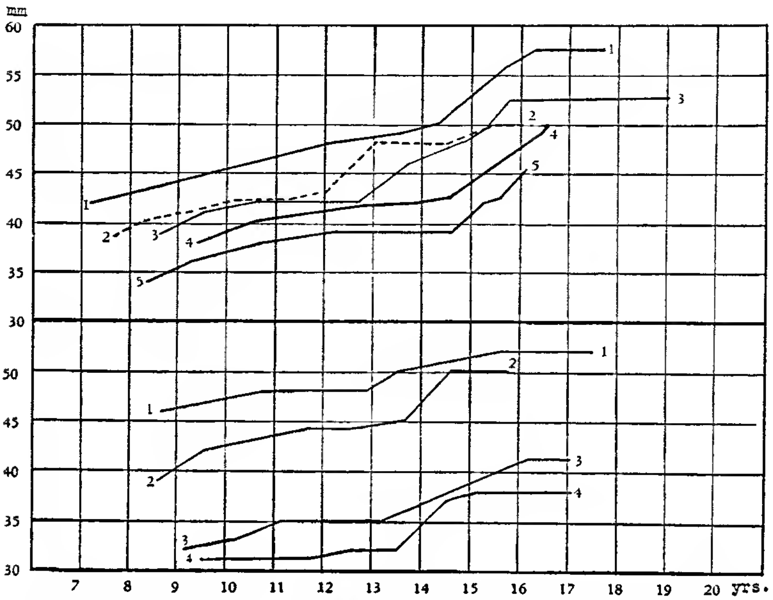

FIG. 4. Individual curves of growth of nasal height in 9 males of the LVD_I series. Above: 1, R.C., No. 84; 2, A.DiM., No. 38; 3, F.W., No. 24; 4, R.H., No. 29; 5, M.H., No. 2. Below: 1, S.B., No. 55; 2, C.S., No. 15; 3, W.S., No. 16; 4, L.F., No. 113.

The age of the spurt in growth of nasal height varies, in a series of 39 boys, from 12 to 18 years. Of this series the empirical mode is 15 years and this is also the empirical mode for spurt in stature-growth of the same boys. Accordingly, the two spurts of growth usually occur at within about 1 year of each other.

The correlation in age of occurrence of the two spurts is 0.795 ± 0.060. This is, of course, a rather high correlation. It may be concluded, consequently, that the same causes, especially hormones, that stimulate growth of the body as a whole stimulate the nose to grow in height. It is interesting to note that an organ thus stimulated to grow

responds in its own way so as to preserve, for the most part, the general bodily proportions. The nature of the growth response depends as much upon the part stimulated as upon the stimulating agent.

As one looks at Fig. 4, one is impressed by the general resemblance in form of the curves of growth of nasal height. They run at different size levels, to be sure. At age 16 they range from 38 mm to 57 mm. Yet they have attained the different end-sizes by nearly parallel paths. No. 2, indeed, stops growing early, so this curve intersects that of No. 3 which keep on growing in height for some months longer. With this exception the relative heights of the noses inside each set remain the same at 16 years as at 9 years. Nasal growth in height proceeds in these cases in approximately the same way.

But this resemblance in growth is not true of all boys of the white race, as appears from Fig. 5. The order from top to bottom at 9 years of 1, 2, 3, 4, 5 has at 15 years be-

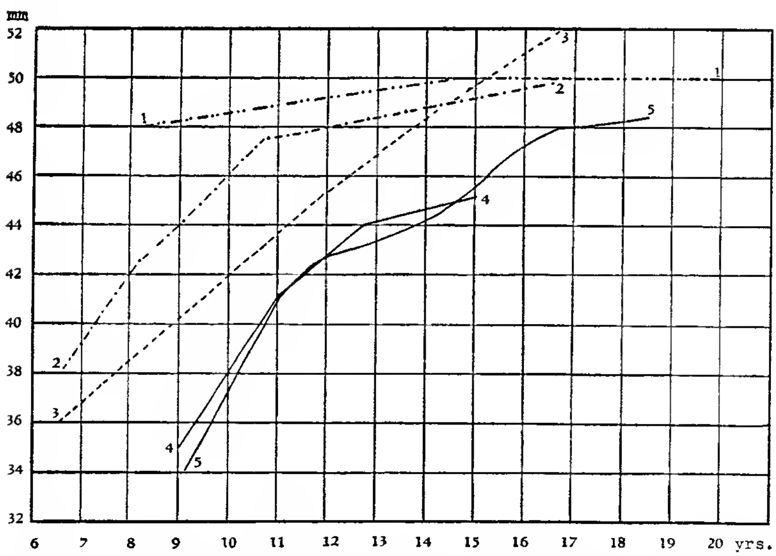

FIG. 5. Individual curves of growth of nasal height in 5 individuals of LVD$_I$ series. 1, I.S., No. 70 (Polish); 2, T.F., No. 27 (Ital.); 3, R.O., No. 109 (Nordic); 4, E.B., No. 51 (Colored); 5, T.V., No. 65 (Ital.), Pl. IX, Fig. 11.

come transformed into the order 3, 1, 2, 5, 4. In No. 3 (of Scandinavian origin) growth proceeds rapidly in nearly straight line fashion from 6½ to 16½ years, increasing from 36 to 52 mm or 1.6 mm per year. On the other hand, No. 1 (Polish ancestry) increases only slowly, but changes its rate of growth by slowing up after 15 years. No. 1 increases from 8 to 19 years only 2 mm or at the rate of less than 0.2 mm per year. No. 2 (Italian) shows a marked break in growth of nasal height at 10½ years, but no adolescent spurt. No. 5 shows several breaks. No. 4 is a mulatto whose curve of growth of nose height, while always low, is located near to that of the Italian, No. 5. A con-

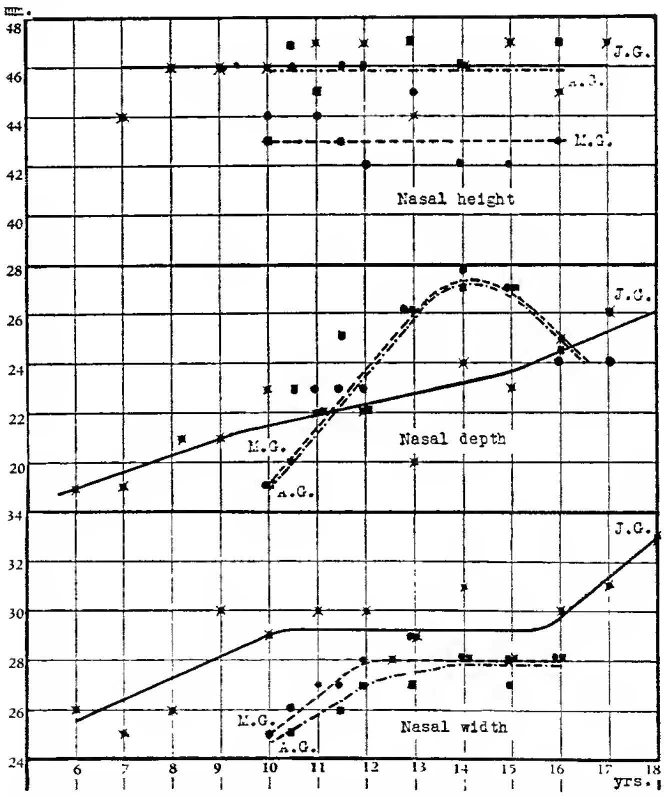

FIG. B. Growth curves of nasal height, nasal depth and nasal width of the G. twins, A.G. and M.G., and their brother, J.G. Scale of millimeters at left.

templation of Fig. 5 enforces the conclusion that one cannot always predict eventual nasal height from size at 9 years. Family, racial and other factors, genetical and others, may influence the result (*cf.* also Fig. 4).

β. Familial.—The influence of the family on growth of nasal height may or may not determine similarity of form. It depends upon whether the different members of the family do or do not receive from their common parents the same genes that control this dimension. In the case of the G. fraternity (Fig. *B*) comprising a boy, and twins who are "identical," all three show a similar horizontal curve of growth of nasal height.

In the case of four brothers of the W. family (Fig. 6) the nasal heights increase roughly in parallel fashion, but

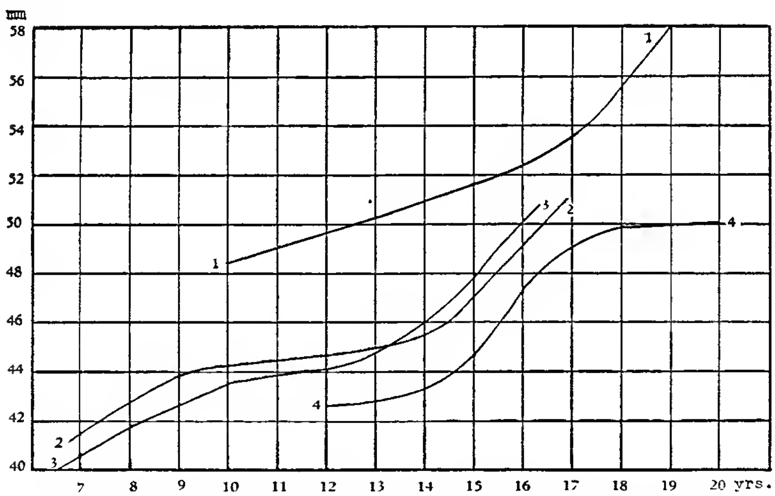

FIG. 6. Individual curves of growth of nasal height in War. family (U. S.), 1, Frank; 2, Stanley; 3, George; 4, William.

at 16 years the height is 5 mm greater in the highest than in the lowest.

One gains the impression that growth slopes are inherited more closely than absolute size.

d. Summary.

From mass statistics it appears that the nasal height follows closely stature height, increasing rapidly before birth, slowing up greatly at 6–7 years, accelerating slightly at 10 years and again still more at about 14 to 15 years. After about 16 years, growth of nasal height is very slow. Nasal height in the female is about 10 per cent less than in the male, corresponding to the smaller stature of that sex.

Between the various social groups, the BOA and Letchworth groups, there is little difference in nasal height, but the male Mongoloid dwarfs have a nose height at 16 years which is about 6 mm or 11 per cent below the BOA standard.

The U. S. and Nordic males despite large stature have the lowest noses. In the Mediterranean male group the nasal height is about 2 or 3 mm greater. In the Negro it is intermediate. Among the females the Negro children seem to have a higher nose than the white children of the same age. Their nasal height stops growing rather early.

In most individuals the nasal heights run parallel courses, with a spurt at adolescence. Indeed, the correlation between the age of occurrence of spurt in stature growth and that in nasal growth is about 0.80. In some cases the curve of growth of nasal height is quite different and runs athwart the more usual curves.

In boys or girls of one family one sometimes finds the growth curves of nasal height to run parallel though they do not coincide. The relations of the nose to body in the family are maintained more closely than absolute heights. In a dwarf girl the growth of nasal height is 23 per cent below standard agreeing pretty well with deficiency in stature.

2. *Growth of Nasal Depth*

a. Definition and Method.

Nasal depth carries two definitions in Martin's Lehrbuch ('28, p. 188). His No. 22 is what we call nasal salient, following the French: "Saillie de la base du nez." Our

nasal depth agrees with Martin's No. 22a, which reads: "Projektivische Entfernung der Spitze der Nase von dem hintersten Punkte des Ansatzes der Nasenflügel an der Wangenhaut". The line of insertion of the nasal wings into the cheek is called the nasal sulcus or hinder wing insertion.

The method of measuring nasal depth adopted in much of our work is two-fold, the two methods checking each other. First, we have used the sliding calipers with the movable arm reversed. Then we hold the end of the graduated stem lightly in contact with the nasal sulcus of the right side, parallel to both ear-eye plane and the sagittal plane, and bring the moveable arm to the apex of the nose. The distance is then read and the operation is repeated on the left side of the nose. The record of the nasal depth is the average of the two measurements. It is, by the way, rather striking to see how often one lateral half of the nose differs by a millimeter or two from the other. The difficulty that is sometimes encountered in using this technique for getting nasal depth is that the free edge of the nasal septum may occasionally be so elevated or depressed distally that the horizontal plane running through the nasal tip may pass above or below the limits of the sulcus. Also, if the apex be thus elevated or depressed there is a tendency of the observer to deviate from the horizontal in measuring the nasal depth.

The other technique, which affords a useful check, is carried out with the subject's occiput in contact with a vertical wall, head held with both eye-ear horizontal and sagittal plane perpendicular to the wall. A narrow mirror placed on the wall across the room from the sitting subject and placed at a height from the floor equal to the height of the subject's eyes, reflects those eyes and helps to hold the head in position. Then with the "depth measurer" of Rickenbach's manufacture, the projective distances of nasal sulcus and nasal apex from the wall are determined on each side and averaged, and the nasal depth obtained by subtraction.

In practice the measurements by the two techniques should agree within 2 millimeters; if not they are repeated until they do. The recorded nasal depth is the mean of the readings by each method.

b. Results.

b′. Mass Statistics.

α. General.—Nasal depth can first be measured when the nasal hillock arises from the undulating plane of the 7 week embryo by the drawing together and fusing of the pair of thickened rings around each nostril (Pl. VI, Fig. 2). Thus there is a time when the outer nasal prominence has practically zero depth. Once established the outer nose becomes constantly deeper until at birth it is, on the average, about 12 mm deep.[1] During the first post-natal year the nose depth grows more slowly. After one year, in the Nordic boy, it grows about 5 mm in 4 years or an average of 1.25 mm p.a. From 5 to 9 years, growth appears to be slow, 1.6 mm in 4 years, or 0.4 mm p.a.; then more rapid, especially in boys at about 12 to 13 years (1.7 mm in 1 year), and finally falls to a rate of about 0.5 mm per year in boys at 17 years (Fig. 7).

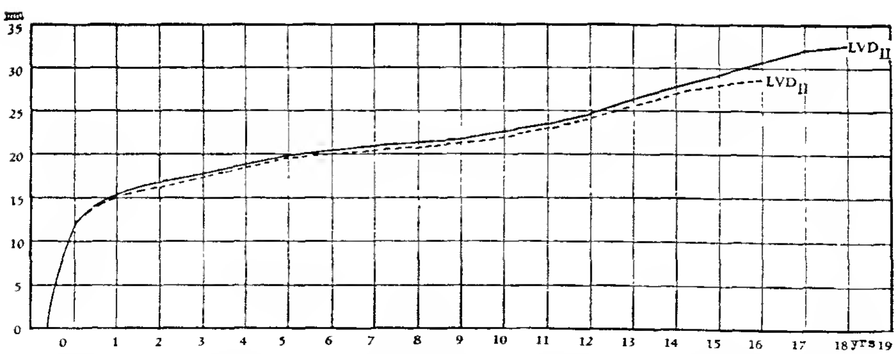

Fig. 7. Mass curves of growth of nasal depth, embryo to maturity, combining data as in Fig. 1. The later ages (6–18) are based on the LVD$_{II}$ series. Male and female.

[1] Fetuses of the 9th lunar month gave a nasal depth of about 9 mm. See Pl. VIa.

β. Sexual.—The curves of growth of nasal depth of the two sexes run in closely parallel courses from about 1 year to 13 years; the nasal depth of the Nordic girl being about 0.5 mm less than of the boy (Fig. 7). This is about 2.5 per cent. There is possibly a slightly higher rate of depth growth in the girl than in the boy at 11 to 12 years. Growth of nasal depth slacks down in the girl at 15 years to about 0.5 mm p.a. At 16 years the nasal depth is about 2 mm or about 7 per cent less in the girl than in the boy, corresponding roughly to the sexual difference in stature.

γ. Social.—The nasal depth of the two groups of children from Letchworth Village is greater, on the average, than the BOA standard by between 1 and 2 mm. Between the ages of 13 and 17 years, indeed, the boys of the Idiot series have deeper noses than those of the LVD_I series of boys, more highly selected for intelligence. The average nasal depth of the Mongoloid dwarfs is 2 to 4 mm less than that of the LVD_I series. The greater nasal depth of idiot children seems to be a real phenomenon (Fig. 8).

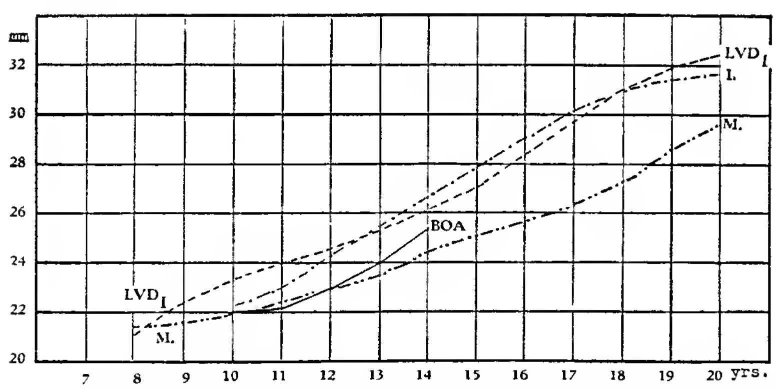

FIG. 8. Mass curves of growth of nasal depth in four social groups, BOA, LVD_I, I and M. U. S. and Nordic. Males only.

δ. Racial.—The nasal depth of the Nordic boys (LVD_I series) measured is (between 10 and 15 years) greater by about a millimeter, on the average, than that of Negroes. The course of development of this dimension in Mediter-

ranean children (Italians and Jews) is somewhat erratic, possibly due to the inclusion of two such racial elements, diverse in the form of the nose (Fig. 9).

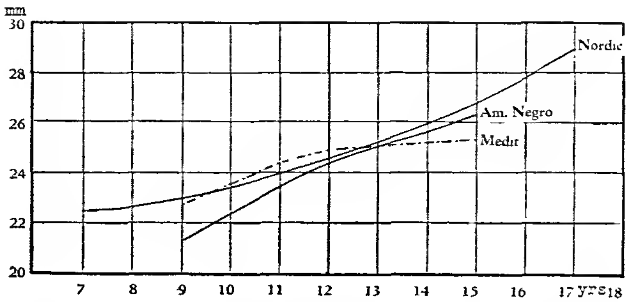

Fig. 9. Mass curves of growth of nasal depth in three racial groups, LVD_I series: Nordic, Mediterranean and Negro. Males only.

b″. Individual Data.

a. Comparative.—Figure 10 shows the course of individual development of nasal depth in a number of white boys of Nordic or mixed U. S. stock.

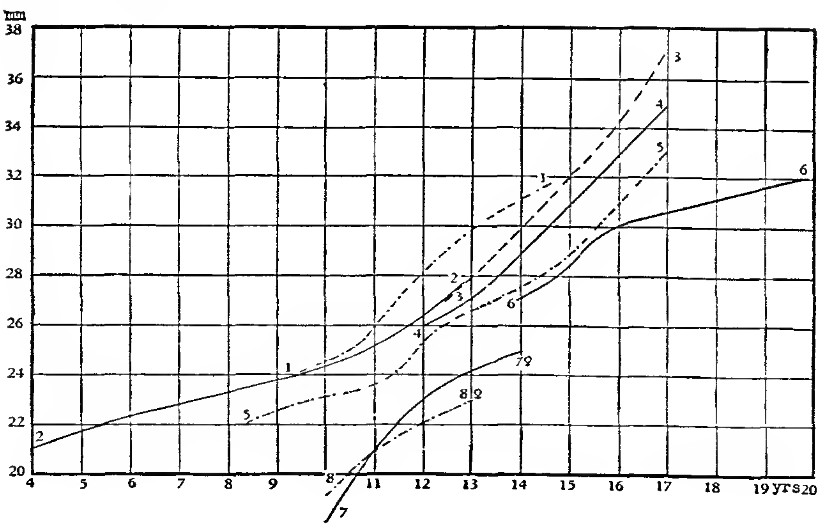

Fig. 10. Individual curves of growth of nasal depth. 1, R.C., No. 201, LVD_{II} (U. S.); 2, G.W., F.S. (U. S.); 3, C.C., No. 26, LVD_{II} (U. S.); 4, W.D., No. 34, LVD_{II} (Ital.); 5, R.B., No. 12, LVD_{II} (U. S.); 6, A.T., No. 1, I (U. S.); 7, H.B., No. 16, LVD_{II} (Colored); 8, L.B., No. 17, LVD_{II} (Colored). Males except Nos. 7 and 8.

In most cases the curve is concave above and to the left during the adolescent period. The velocity of growth of nasal depth is greatest in two cases between 11 and 12 years, but in one of these again at 15–16 years; in three cases it is around 15 years, about the period of the adolescent spurt. The increase then is about 2 mm p.a. This is a larger increase than the mass curve shows. This fact again illustrates the principle that the mass curves do not adequately show how a child, or any part of a child, grows.

Some of these curves show that the velocity of growth in nasal depth has not yet slackened at 17 years. It is a matter of common observation that the nose depth frequently continues to increase well into the third decade.

The lower part of the chart shows the curve of growth of nasal depth for two colored girls. These show a small nasal depth, but one of them has a markedly high velocity of nasal growth.

β. Familial.—Figure 11 shows the growth of nasal depth

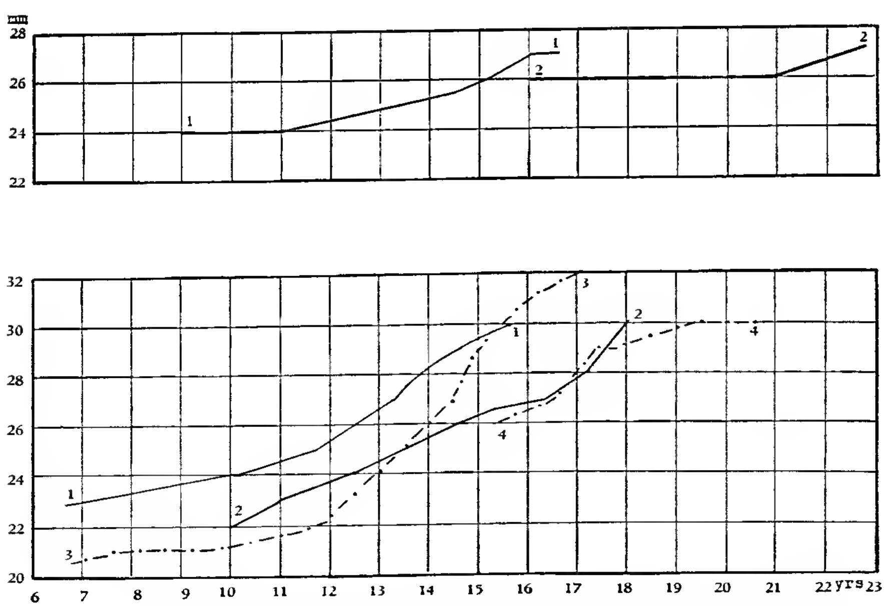

FIG. 11. Individual curves of growth of nasal depth in War. (U. S.) family. Above: 1, Mary; 2, Bertha. Below: 1, George; 2, Frank; 3, Stanley; 4, Fred.

in the War. family. There is of course no particular reason for anticipating that in all the siblings nasal growth will take place in the same way; nor does it do so in this case. In Nos. 2 and 4 (below) the growth curves nearly coincide from 15½ to 18 years. The spurt of growth occurs in both at this time. No. 1 runs roughly parallel to No. 2 from 10 to 13½ years; but the two curves gradually diverge from thence on. No. 1 shows the greater velocity of growth in depth. No. 3 is more aberrant. After 12 years, nasal depth increases at the rate of 2 mm p.a. as contrasted with 1.5 mm in the next most rapidly growing nose. Consequently, from a lowly position at age 12 it comes to be the deepest nose at 16 years.

The two girls of this family (above) differed so widely in age that close comparison is impossible; at 16 years the nasal depths are only 1 mm apart; whereas the greatest difference for the boys at this age is 4 mm.

Figure 12 shows (Nos. 1 and 6, below) the comparative growth in nasal depth for 2 boys of the Sti. family. The growth curves run a roughly parallel course but nearly 4 mm apart. In the upper part of the figures are growth curves for 2 sisters of this family. No. 1's growth curve runs closely parallel to her brother's No. 1, below, but about 4 mm lower, as befits her sex. No. 2's growth curve is more like her brother No. 6's; and interlaces with it.

The 3 Mea. boys in Fig. 12, Nos. 3, 4, 5, are not so far apart from each other in nasal depth growth as are the two Sti. brothers. The curves of George (No. 4) and William (No. 3) practically coincide at 10½ to 13 years. Then for a time William's nose depth grows less rapidly than George's, but the two curves come together at 20 years. William comes a year later with his adolescent nasal depth spurt than George; and that fact keeps the noses unlike for a time.

In the Mea. girl, Bertha (No. 2) the growth of nose depth is quite like that of her brother George (No. 4), ex-

cept that between 13 and 16 years the sister's nose depth is about 2 mm the greater.

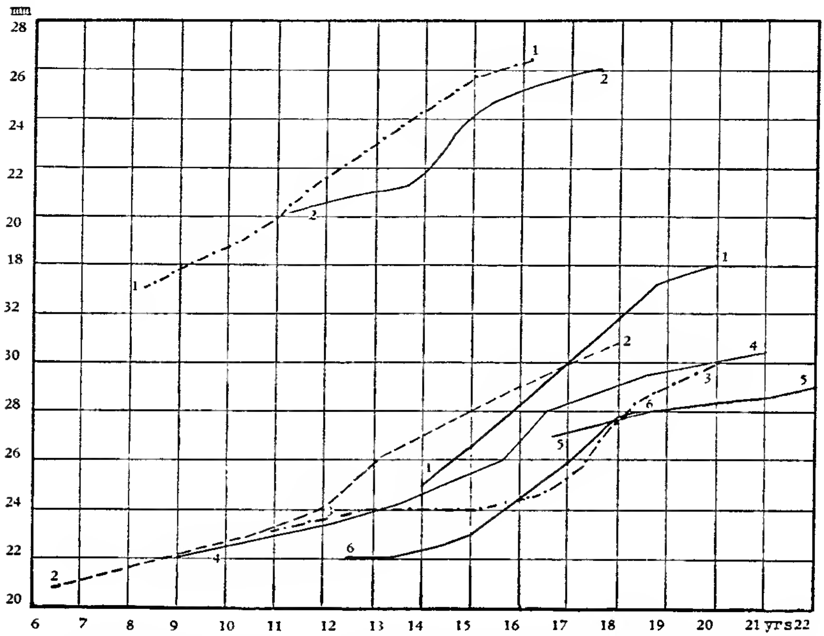

FIG. 12. Individual curves of growth of nasal depth in Mea. (U. S., part Indian) and S. (U. S.) families. Above: 1, Myrtle S. ♀; 2, Edith S. ♀. Below: 1, Claude S ♂; 2, Bertha M. ♀; 3, William M. ♂; 4, George M. ♂; 5, Henry M. ♂; 6, Lloyd S. ♂.

γ. Special cases. Figure *B* (page 196); Pl. V, 3 show the practical identity of the nose depths of the twin sisters, G., at the given ages; the brother's curve takes a somewhat different course.

Figure 13 gives curves of growth of nasal depth for some special cases. Thus, Nos. 3, 5, 6 are dwarf girls. No. 6 is probably a sub-pituitary dwarf; 5 is apparently an ateliotic, while No. 3 is a girl of retarded physical and mental development. The striking thing about all three curves is the great spurt of growth about 14 and 15 years respectively—an age at which most girls have nearly stopped growth. But these girls were growing fast in

stature at 15 and 16 years. These dwarfs are examples of very retarded development.

Curve 2 is that of a female cretin who is rapidly outgrowing her cretinous features. The growth in nasal

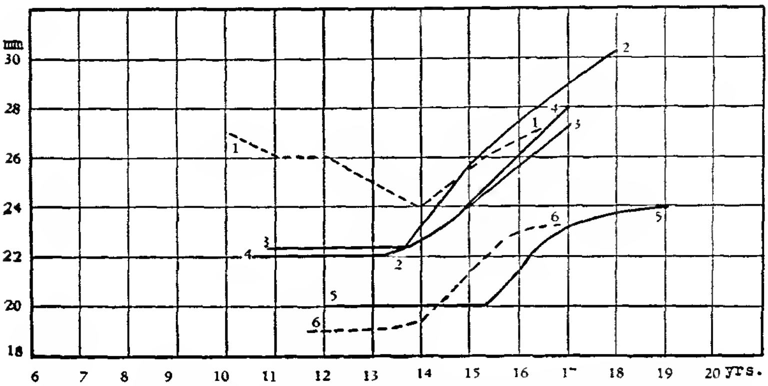

FIG. 13. Individual curves of growth of nasal depth in six special cases: 1, M. Gio., No. 95 (Ital.), acromegaly; 2, C. Blo. ♀ (U. S.), cretin; 3, M. Coo. ♀ (U. S.), ateliotic? dwarf; 4, V. Rus. (Ital.), microcephalic; 5, A. Wal. ♀ (U. S.), ateliotic; 6, L. Sch. ♀ (Jew), pituitary dwarf.

depth is almost cataclysmic, 2.4 mm p.a. from 13½ to 15 years.[1]

Curve 4 is that of a microcephalic boy (V.R.) who is only slightly below the average stature of Italians. He shows precisely the form of nasal growth curve that 3 shows.

Finally, curve 1 is that of an acromegalic, *i.e.*, a hyperpituitary case; a small giant, whose stature is reduced by a marked stoop. This is a remarkable curve of growth, in that it shows a decrease in the prepubertal period from 27 to 24 mm followed after 14 years by an increase to 27 mm again. The explanation for this slump of 2 mm at 14 years is unknown. The fact may be related to the dissymetry of nose depth as measured on the two sides.

1 This girl, born March 8, 1918, was put on thyroid therapy from 1927–1931 (ages 9–13), but the dosages were not regular and there was an intermission of a few months during which school work deteriorated. From about 14 to 17 years she has received 2 grains daily of desiccated thyroid; more irregularly in the last two years.

3. *Growth of Nasal Width*

Nasal width is the distance between tangents to the alæ (wings) of the nose at their greatest breadth. There is no difficulty about landmarks. The measurement is made when the nostrils are in a resting position.

The nasal width is largely, if not chiefly, determined by the form of the paired cartilages which enclose, in horseshoe fashion, the pair of nostrils. The structure of the alar cartilages has been well described by Spurgat ('96) and by Schultz ('18, pp. 329–338; '35, pp. 208–212) both for Whites, Negroes and the gibbon, orang-utan, chimpanzee and gorilla. Weninger ('26) and still earlier Hovorka ('93) and Virchow ('12) have analyzed these structures. The development of the nasal cartilages in Primates has been exhaustively studied by Wen ('30) following earlier work by Peter ('13, pp. 81, 82) and others. The nasal wing proper is a fold of the skin containing the mucosa, dilator naris posterior muscle, subdermal and epidermal layers.

The nasal wings have an interesting pedigree. They are often more highly developed in the anthropoid apes than in man. In the gorilla and orang they form thick folds; in the chimpanzee they are broad and flat (Pl. 1).

Ontogenetically these folds arise very early (10 mm long embryo) as thickenings lateral to the openings of the nasal sacs (eventual nostrils) (Pl. VI, 2, 3).

Physiologically they surround a shallow "vestibule" at the beginning of the nasal canal. Their principal significance in man, as in the gorilla, would seem to be for ornamentation—added sculpture upon the lateral nasal surface —and for the expression of the emotions (Pl. II).

a. *Mass Statistics.*

α. *General.*—Figure 14 shows the "curve of growth" of nasal width based on means at different ages. Starting at 3 or 4 mm on first appearance of the external nose, absolute breadth increases rapidly to about 18 mm in white babies

at birth,[1] growing 14 mm in 8 months or at the rate of 21 mm per year. Thereafter growth proceeds at an ever

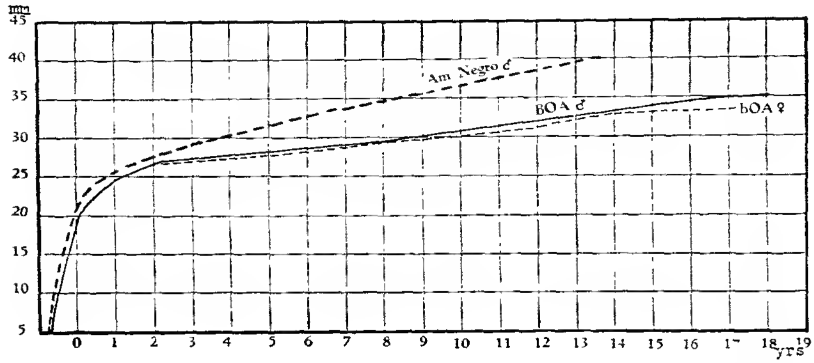

FIG. 14. Mass curves of growth of nasal width, embryo to maturity, combining data as in Fig. 1. The later ages (6–18) are based on BOA data, for whites; LVD$_I$ for Negroes. Male and female white; and Negro ♂.

slower rate; during the first six postnatal months, at the average rate of 6 mm per year, in the second half of the first year at the average rate of about 4 mm per year; during the second year at the average rate of 2 mm per year. After two years and until six years the growth of the nose in width is very small, 0.5 mm per year. After that, growth proceeds at the rate of about 0.6 mm p.a. to maturity. Wiazemsky ('07, p. 78) notes the increase in rate of growth of width of the nose at puberty.

β. Sexual.—The figures just given are for Nordic boys (of BOA and LVD series) up to 17 years. They run close to (within 1 mm of) the means for U. S. A. private school children as given by Gray and Ayres ('31). There is reason for concluding, from all of these data, that nose breadth of boys increases little after 17 years.

In girls the growth curve of average nasal width from 2 years onward usually lies at first a fraction of a millimeter below that of the male, as Niggli-Hürliman ('30, p. 84) also found. At about 12 or 13 years the nose breadth

[1] Houzé (1885) finds for Belgian neonates a nasal width of 19.0; Blind (1890) for Münich neonates, 20.9 mm.

is about the same in both sexes. Thereafter the nasal width of the male grows more rapidly than that of the female and eventually comes to exceed it by 1 to 3 mm. The difference is of the order of 5 to 8 per cent.

Observations by others on nasal width of adult Europeans gives about 35 mm for males and 32 mm for females.

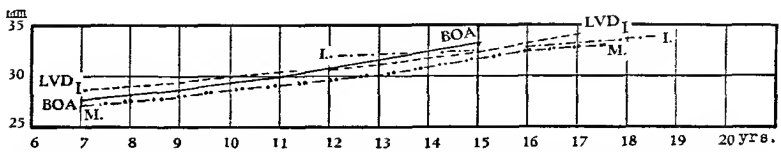

FIG. 15. Mass curves of growth of nasal width in four social groups: BOA, LVDI, I, M., U. S. and Nordic males only.

γ. *Social.*—As Fig. 15 shows, the BOA (standard) boys start off at 7 years with an intermediate average nasal width of 27.6 mm. The nasal width grows gradually, with increasing speed for a time at beyond 12 years, passes the LVDI series at 12 years so that, at 15 years, the nose is 0.8 mm broader, on the average, than that of the LVDI and I series.

The LVDI series starts at 7 years with a mean nose which is 0.7 mm, on the average, wider than the standard, while at 16 years it is 0.8 mm narrower than standard.

In the I series the nose width starts high, averaging 32 mm at 12 years. But mean growth is very slow and at 17 years the mean nasal width is 0.7 mm less than in the LVDI series. There seems to be a relatively weak energy of growth of the nasal width in the I series.

In the M series the nasal breadth is pretty uniformly 0.5 to 1.5 mm (or 3 to 10 per cent) less than standard. The narrow nose is thus in keeping with the low stature of this dwarfish group.

δ. *Racial.*—As Fig. 16 shows, in the males of the LVDI U. S. and Nordic groups the average nasal width at 8 years is greater than that of the Mediterranean group, and by 15 years it is 1.5 mm greater.

The mean nasal width of the American Negro male nose is on the average of our series extraordinarily high, being at 9 years 35.5 mm or 5 mm greater than that of the Nordic

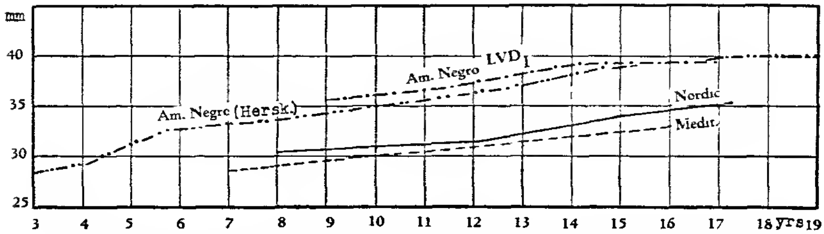

FIG. 16. Mass curves of growth of nasal width in four racial groups: Nordic, Mediterranean, Negro (all of LVD$_I$ series) and, for comparison, Negroes of Herskovitz. Males only.

group. Growth seems slightly accelerated at 12–13 years, but nearly ceases after 14 years, when it is about 4 mm greater than standard. Herskovits ('27, p. 309) finds mean Negro nose width in his series to increase 2 mm after 14 years, reaching about 39.5 mm at 17 years as contrasted with 35 mm for U. S. mixed and Nordics. Davenport and Steggerda ('29, p. 175) find an average nasal width for adult Blacks of Jamaica: males, 45.82 ± 0.26; females, 40.72 ± 0.27. The Whites of the same region had respective mean nasal width of 34.90 ± 0.24 and 32.29 ± 0.26 while the Browns occupy an intermediate position, much nearer to the Blacks than Whites. Nasal width is one of the most variable of nasal dimensions and is an important racial differential. Table 2 gives some mean nasal widths for various "races" of mankind, based mostly on rather recent data.

b. Individual Data.

a. Males.—The curves of individual growth in nose width (Fig. 17) increase somewhat steadily from 5 to 8 years onward. An upward spurt is apt to occur at 12 to 14, even to 16 years, doubtless in relation with the adolescent spurt of growth. A considerable range in the growth curve

TABLE 2

MEAN NASAL WIDTHS IN ADULTS OF VARIOUS RACES

Race	Author	Nasal Width, mm	
		Males	Females
Fehmaraner . . .	Saller, '30, p. 126	33.6±0.5	31.9±0.3
Norwegians. . . .	Bryn, '30, p. 12	34.0±0.2	
U. S. A. Whites (cadavers).	Todd and Lindala, '28, p. 115	34.9±0.2	31.6±0.6
Danes.	Hannesson, '25, p. 117	35.0	
Icelanders.	Hannesson, '25, p. 117	35.3±0.1	
Norwegians (Holandsdal) . . .	Schreiner, '30, p. 36	35.6±0.3	32.8±0.3
Norwegians (Valle). . . .	Schreiner, '30, p. 36	36.14±0.22	33.37±0.24
Old Americans. . . .	Hrdlička, '25, pp. 248, 9	36.1(30–43)	32.5(28–38)
Smith College students.	Steggerda et al, '29, p. 220		32.3±0.1
Sudetenlanders (Czech).	Knöbl, '31, p. 47	36.2±0.2	33.1±0.2
Runö, Swedes. . . .	Hilden, '29, p. 90	37.0±0.3	33.8±0.3
Faroe Is., Danes	Jorgensen, '02 (teste Saller)	37.1	32.9
Javanese (Selahawi)	Nyèssen, '29, p. 92	37.3±0.6 (31–44)	33.8(29–45)
Amer. "Negroes" (cadavers).	Todd and Lindala, '28, p. 115	42.4±0.2	39.5±0.5
Ashanti negroes. .	Rattray, '23, p. 335*	42.5	36.8
Ekoi negroes. . . .	Mansfield, '08*	43.9	
West Coast Africans	Weninger, '27, p. 69	44.2±0.2 (37–53)	
Hawaiians.	Dunn, '28, p. 118	44.2±0.2	40.9±0.4

* Teste Todd and Lindala, '28, p. 85.

at a given age, *e.g.* 13 years, is seen, namely from 29 to 36 mm, or 7 mm. This is a range of 23 per cent on a basal width of 30 mm.

In general, growth in the different individuals seems to be roughly parallel. However there are differences. Thus in curve 9 (No. 2, M.H.) the nose width rises from 28.5 to 35 mm in 8 years, or at the rate of 0.81 mm p.a. On the other hand, curve 3 (No. 10, J.C.) rose only from 34 to 38 mm in 10 years or at the rate of 0.4 mm p.a. In curve 4 (of No. 30, C.D.) the nose increased in width from 32 to 39 mm in 12 years, or 0.6 mm p.a.

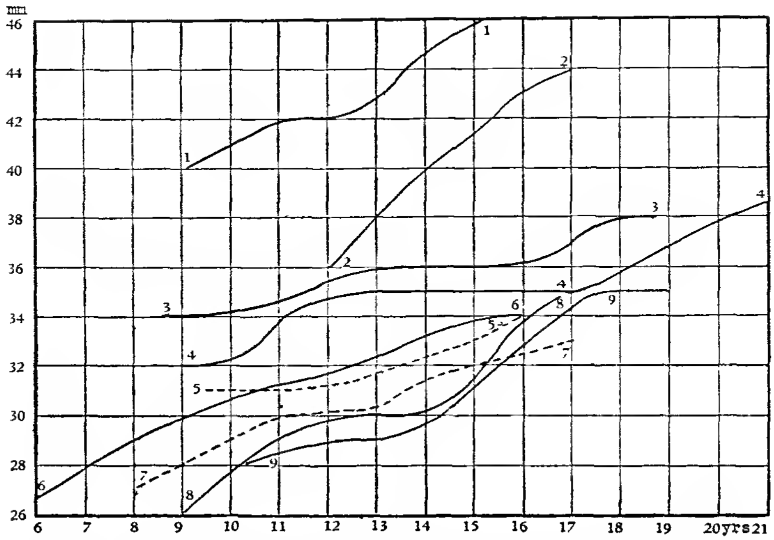

FIG. 17. Individual curves of growth of nasal width in LVD$_I$ series. 1, C.H., No. 26 (colored); 2, G.W., No. 111 (colored); 3, J.C., No. 10 (Nordic); 4, C.D., No. 30 (U. S.); 5, M.J., No. 55 (U. S.); 6, C.H., No. 83 (U. S.); 7, R.B., No. 12 (U. S.); 8, A.M., No. 7 (U. S.); 9, M.H., No. 2 (Nordic). Males only.

The age of still-stand in nasal width varies from 15½ to 17½ years. But No. 4's (C.D.'s) nose is still gaining width at 21 years.

The growth of the Negro nose in width occurs, of course, on a much higher plane. In the case of curve 2 (G.W.), width increases from 36 mm at 12 years to 44 mm at 17 years, or 1.6 mm p.a., and in this particular individual there is only a slight spurt of nasal width growth, viz., at 15 years.

In the case of curve 1 (No. 26, C.H., colored) the nasal width increases from 40 mm at 9 years to 46 mm at 15.2 years, or about 1 mm p.a. Here there is a decided spurt of growth at 13–14 years.

Thus it appears that one reason why the nose of the adult Negro is so broad is that it not only starts early, some months before birth, to grow faster in width than depth, but it continues after birth to grow in width much faster than the nose of whites. That is, whereas at birth

the width of the Negro nose is about 13 per cent in excess of the white nose, at 13 years it is 21 per cent and still vigorously growing.

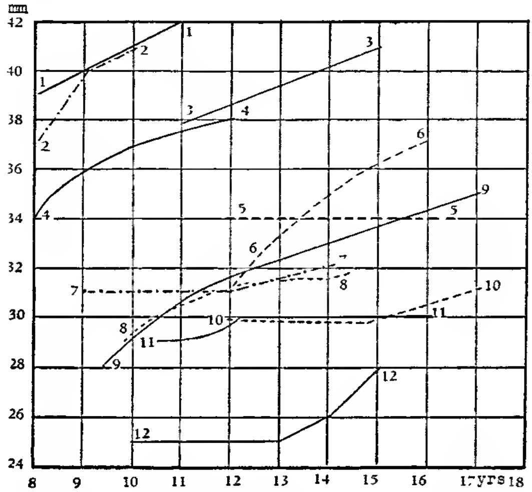

FIG. 18. Individual curves of growth of nasal width, LVD$_{II}$ series. 1, I.W., No. 327 (colored); 2, G.W., No. 326 (colored); 3, L.D., No. 135 (colored); 4, H.B., No. 16 (colored); 5, B.A., No. 3 (Ital.); 6, M.H., No. 51 (U. S.); 7, M.B., No. 8 (Ital.); 8, H.C., No. 21 (U. S.); 9, A.S., No. 101 (Hungarian); 10, E.C., No. 25 (U. S.); 11, G.H., No. 52 (U. S.); 12, N.C., No. 27 (Nordic). Females only.

β. Females.—As Fig. 18 shows, the nasal width of girls increases somewhat steadily during youth and adolescence. Upward spurts are less uniformly present than in boys, and occur earlier. Thus in the curves shown the maximum increases are usually around 13 or 14 years. There is a considerable range in width at 13 years, from 25 to 34, or 9 mm; at 16 years, also 9 mm. Some curves show very little growth (curve 11, G.H.), only 1 mm in 5.5 years, or at the rate of 0.2 mm per year. On the other hand, in curve 6 (M.H.) the nasal width increased 6 mm in 4 years, or 1.5 mm per year. The nasal width of adult females is less than that of adult males, partly because the nasal width increases more slowly, especially after 15 years when growth has largely stopped in girls but not in boys. The age of still-

stand in girls is at about 17 years, as opposed to 17½ years in boys.

γ. *Racial.*—The growth of nasal width in four Negro girls is indicated at the upper part of Fig. 18. As with Negro boys the nasal width even in childhood, is already in a different class from that of the whites. Also, the rate of growth is high. Thus in two of the girls whose growth curves are given, the increase in nasal width is at the rate of 1.0 mm per year.

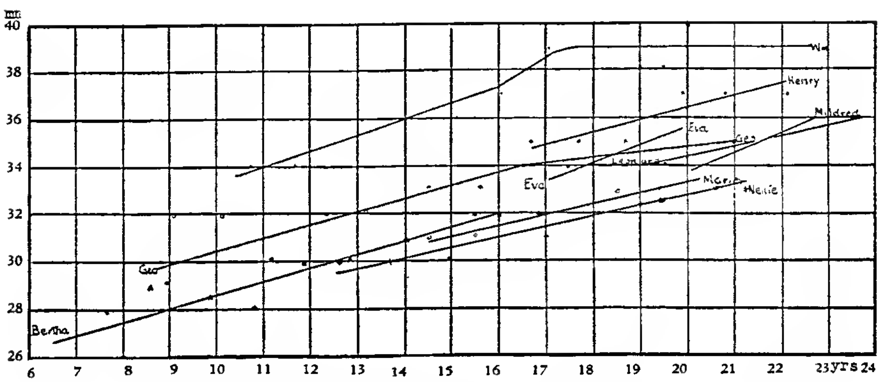

FIG. 19. Individual curves of growth of nasal width in 9 members of the Mea. fraternity. Actual measurements recorded are indicated.

δ. *Familial.*—Figure 19 gives the changes in nasal width during development of the 9 members of one (Mea.) family all at Letchworth Village. The curves do not coincide, but they run strikingly parallel courses.

ε. *Special cases.*—Figure *B* (page 196) shows curve of nasal width in a pair of (by numerous criteria) monozygotic twins. The similarity of the curves speaks for itself. The great similarity of the nasal width, seen in the frontal portraits of the girls, is due to the fact that the nasal width has grown along the same lines. The reason why it has followed similar lines is doubtless because the girls possess similar genes and cytoplasm.

Figure 20 shows the growth of nasal width in some special cases. Curve 2 is of V.R. a microcephalic, of me-

dium stature. His nasal width is large. It shows two spurts of growth, at 13 and 17 respectively. The other three curves (of L.S., A.W. and M.C.), are all of dwarf

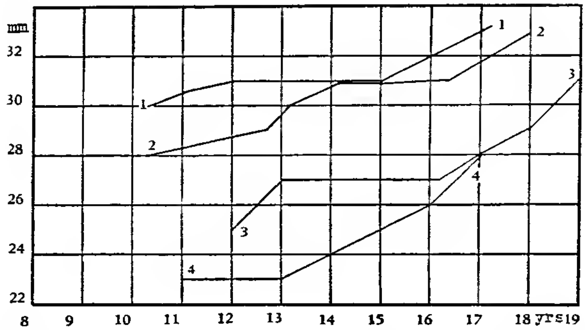

FIG. 20. Individual curves of growth of nasal width in special cases. 1, L. Sch. ♀ (Jew), pituitary dwarf; 2, V.R. ♂ (Ital.), microcephalic; 3, A. Wal. ♀ (U. S.), ateliotic; 4, M. Coo ♀ (U. S.), ateliotic? dwarf.

girls who, however, have taken a spurt of growth at puberty. Curve 1 is of a pituitary dwarf (L.S.); the other two are of ateliotics; in all nasal width is growing rapidly at 17 years, at an age when in normal girls this dimension has stopped growing.

4. *Growth of the Nasal Salient*

a. Definition.

The nasal salient is the distance, measured parallel to the Frankfort horizontal, from apex of the nose to union of upper lip and nasal septum. It is defined by Martin ('28, p. 188): "Tiefe der Nase (fälschlich Höhe der Nase, Nasenelevation; saillie de la base du nez; nasal depth): Projektivische Entfernung des Subnasale vom Pronasale. Stangen-oder Gleitzirkel mit verstellbaren Armen."

The foregoing directions are inadequate. While the nasal apex offers no difficulty the determination of the place of union of upper lip to nasal septum does at times offer a great difficulty. While in many races of mankind, especially in Negroes (because of fatty deposits in upper lip,

Pl. IV, Fig. 8), in Australian aborigines (Pl. XVIII, 4, 6), in most Asiatics (Pl. X), and in some Amerindians (Pl. XVI, 4), the septal margin forms with the sagittal line of the upper lip a fairly sharp angle, in many Nordics this is not the case. The union may be a curve of greater or less radius; the lip itself may be curled so as to be concave in front. Indeed, in extreme cases (Pl. III, Fig. 3) it is impossible to tell where septum ends and lip begins. Under these circumstances it may be best not to attempt the measurement of so ill-defined a dimension.

b. Results.

b′. Mass Statistics.

α. General.—Starting at 2.5 mm, at about 4.5 months after conception, as determined by measurements made at the Carnegie Laboratory of Embryology, courtesy of Dr. George L. Streeter, the curve of growth takes a strikingly

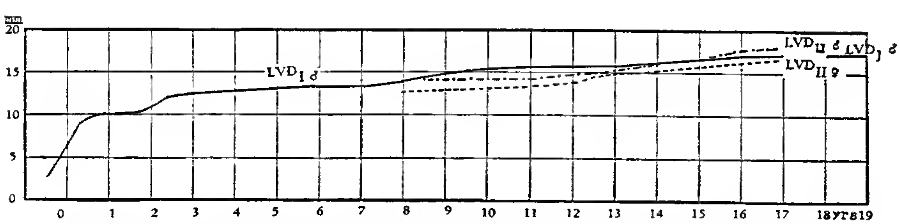

FIG. 21. Mass curves of growth of nasal salient, embryo to maturity, combining data as in Fig. 1. 5–9 years LVD$_I$ series, ages 10–17 from LVD$_{II}$ series. White, U. S. Males and females.

irregular course (Fig. 21); periods of slow growth alternating with periods of very rapid growth. There is a rapid period from 4 months before to 4 months after birth. Between 4 months and 1.5 years the rate of growth of the nasal salient is very slow or reduced to zero, for a year. From 1.5 to 2.5 years rate of growth of nasal salient is rapid up to 3 mm p.a. It slows up from 3 to 7 years to a rate of 0.2 mm p.a. There is a spurt from 7 to 10 years of 0.7 mm p.a. and another from 13 to 16 years, of about 0.5 mm p.a.

These periods of rapid growth correspond to the juvenile and adolescent spurts of general growth. It is to be kept in mind that these are not the rates with which the nose grows in the individual at these ages, but the rate of increase of the mass curve of nasal salient, which is a very different matter. The course of the curves of individual growth in nasal salient is considered below.

β. *Sexual.*—As Fig. 21 shows, already at 8 years, the nasal salient of the average girl of our LVDII series is about 1.5 mm less than that of the average boy of that series. The growth curves tend to approach up to 13 years (to about half a millimeter) and then to diverge again until at 17 years the mean saliency of the girl is about 1.5 mm less than that of the boy in the LVDII series. The approach of the curves at 13 years is doubtless due to the earlier spurt of general growth in girls.

γ. *Social.*—The curve of mean nasal salience of the BOA boys of Nordic and U. S. origin rises slowly from 14.7 at 7 years to 15.3 at 12.5 years and then more rapidly to 15

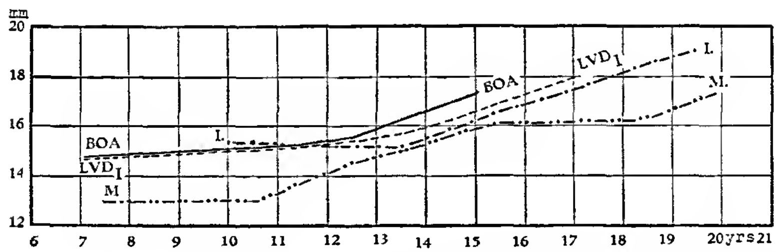

FIG. 22. Mass curves of growth of nasal salient in four social groups, White, U. S.: BOA, LVDI, I, M. Males only.

years (Fig. 22). The curve of Nordic LVDI boys runs close to standard to about 12 years and thereafter falls below the standard curve, since the adolescent spurt of the LVD boys is less vigorous than standard. A mean nasal salience of 18 mm is achieved at 17 years. The curve of the mean I series seems, at 10 years, to lie above that of the standard. In my curves a decussation takes place at 11½ years and

thereafter the curve of the I series lies about 0.2 to 0.4 mm below that of the LVD series. The I curve of nasal depth (Fig. 8) also crosses that of LVD_I, only at 18 years of age.

Of the Mongoloid dwarfs the mean curve lies below all the others, though it approaches very close to the I curve at 14 years. There is an apparent cessation of growth of nasal salience in these Mongoloids at 16–18 years. This may be due to a thickening of the upper lip as the hair follicles of the moustache enlarge.

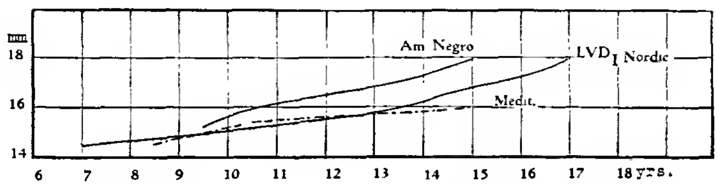

FIG. 23. Mass curves of growth of nasal salient in three racial groups; LVD_I and LVD_{II} series: Nordic + U. S., Mediterranean, Amer. Negro. Males only.

δ. *Racial.*—Figure 23 gives the mass curves of growth of nasal salience in the U. S. and Nordic, Mediterranean and American Negro groups. On the whole, the Nordic group occupies an intermediate position between the small salience of the Mediterraneans (after 13 years) and the markedly more salient nose of the American Negro. On the other hand, the Nordic curve stands highest in nasal depth; *e.g.*, the Nordics have in relation to depth, on the average, a less projecting nose than the Negro (Pl. III, Fig. 2). (See page 257 for an account of the ratio of nasal salience to depth.)

The nasal salience has been little measured in the different races of mankind. A table in Martin ('28, p. 561) gives mean "Nasentiefe" varying from 13.2 in the Mawambi-pigmy adult males to 26.0 in Polish Jews. These latter data appear to me to be wholly unreliable. In my experience I have nowhere attained means in whites at 18 years of over 20 mm. On the other hand, Weninger ('27, p. 70) finds for French West Coast adult Negroes a mean salience of 18.93 which agrees with our data.

b″. Individual Data.

α. Comparative.—Growth of nasal salience is subject to great individual variation of a type which is not revealed by the mass statistics; for the former, as is so often the case, are submerged in the latter.

In males there can be recognized a juvenile maximum of 14 to 17 mm at 10 to 12 years, followed usually by a depression to 12 to 15.5 mm two or three years later. This is followed by a rise often to 19 mm or more (Fig. 25).

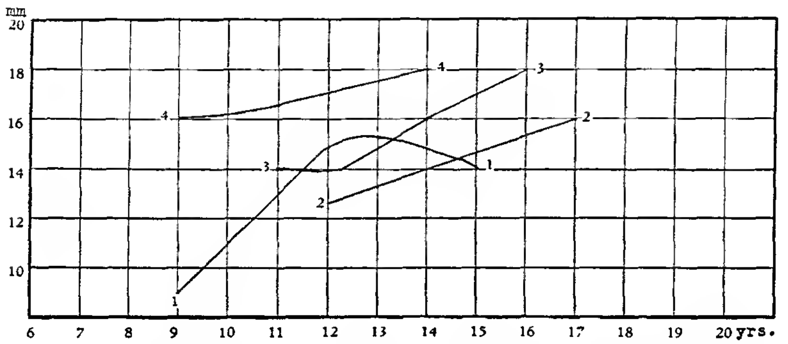

FIG. 24. Individual curves of growth of nasal salient. Females of LVD_{II} series. 1, H.C., No. 21 (U. S.); 2, C.M., No. 74 (U. S.); 3, V.S., No. 102 (U. S); 4, MB, No. 8 (Ital.).

In individual girls, on the other hand, the growth of nose salience is nearly always consistently upwards (Fig. 24, one exception, No. 1).

If a suggestion as to the meaning of the irregularities of the curve of growth of the nasal salient is permissible

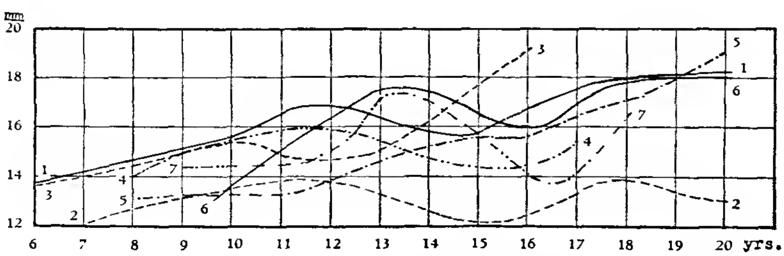

FIG. 25. Males of LVD_{I} series. 1, E.H., No. 1 (U. S.); 2, M.H., No. 2 (Nordic); 3, W.M., No. 8 (U. S.); 4, I.J., No. 18 (U. S.); 5, F.W., No. 24 (Nordic); 6, J.C., No. 10 (Nordic); 7, C.D., No. 30 (U. S.).

at this place, it would be that the absolute reduction in salient at 14 or 15 years in the male is due to a thickening of the upper lip at puberty. This is associated with the great enlargement of the hair follicles there. Possibly the flattening of the average curve of growth of nasal salience at 5 to 7 years may be to a certain extent dependent upon the thickening of the maxillæ as the permanent incisors are developing a year or two before they are cut.

It will be observed that not all of the curves of development of nasal salient in the boys show a reduction in this dimension at puberty. For example, No. 5, F.W., does not. In his case the teeth are not quite so large as in the case of No. 2, M.H. For in No. 2 the first and second incisors have breadths respectively of 8 and 7 mm; in the case of No. 5 the breadths are 8 and 6 mm respectively.[1]

β. Familial.—Figure 26 shows the individual curves of growth of nasal salience in one family. The War. group consists of 5 boys of which the curve of No. 5 (Stanley) is

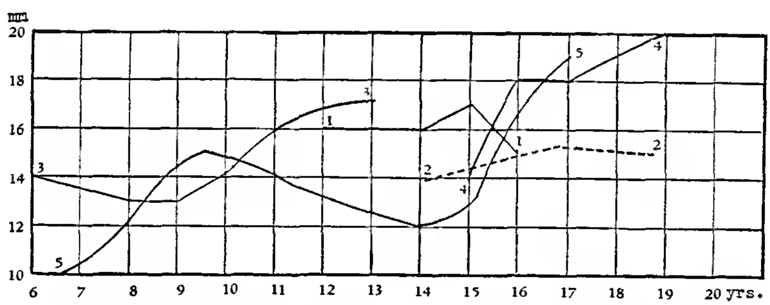

FIG. 26. Individual curves of growth of nasal salient in War. family. U. S. white. 1, William; 2, Frank; 3, George; 4, Fred; 5, Stanley.

[1] That the development of the permanent incisor teeth play an important rôle in the development of the pre-maxillæ and maxillæ is shown in striking fashion in the anthropoid apes as studied by Krogman ('31, a, b, c). In the gorilla the great forward development of the maxillary region is shown in Krogman's Figs. 4 and 5, being the age of development of the permanent incisors (which erupt shortly after M_1; compare Krogman, '31, b. p. 326). In the chimpanzee the great forward movement of the maxilla is between Nos. 51390 and B 409, the latter having just erupted the incisors (Fig. 6). In the orangutan the first great advance of the maxilla is from stage of 17350 ♂ to B 1024 ♀ in which latter skull I_1 is erupting. This process both in anthropoids and in man tends to carry the subnasale point toward the nasal apex. See also Adams, 1917, whose facial curves of children show the same thing.

fairly complete. This shows a marked depression (loss of 3 mm salience) at around 14 years; from which there was complete recovery. No. 3, whose nose has a large salience at 13 years, appears to have had a slight juvenile reduction in growth rate of salience during the 9th year. There is a difference between these boys in salience at 13 years of 5 mm. The nose of No. 2 appears to have met with an accident to the tip, so that the form of his curve of nasal salience is not normal (Pl. VII, Fig. 13). Nos. 4 and 5 have parallel courses in the 15th year. There is no obvious family resemblance in these curves.

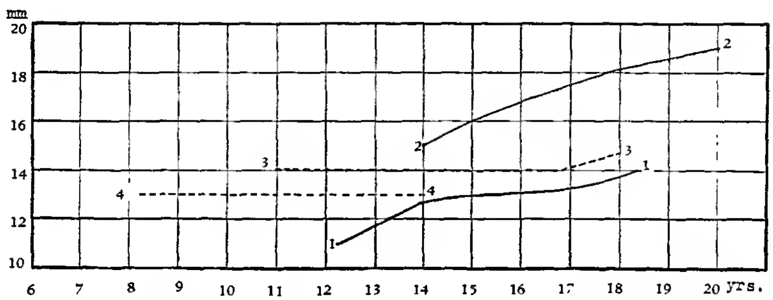

FIG. 27. Individual curves of growth of nasal salient in four members of the Sti. family. U. S. white. 1, Lloyd; 2, Claude; 3, Edith; 4, Myrtle.

Figure 27 shows the curve of nasal salience for all four members of the Sti. family. The curves of the two boys run somewhat parallel courses, 3 to 4 mm apart. The curves of the two girls run parallel courses about 1 mm apart. It will be noted that the growth of the nasal salience of the girls is extremely slow and shows little change between 9 and 17 years.

5. Growth in Depth of Nasal Root

a. Definition and Method.

The nasal root is the lowest (least salient) part of the bridge of the nose: the part just below the level of the brows. The depth of the nasal root is the distance from the bridge of the root to the inner canthus of the eye, meas-

ured parallel to the sagittal plane. It is the "Höhe in Bezug auf d. inneren Augenwinkel," in Martin's ('28), figure on page 558.

This dimension has a particular interest in anthropology. It is very variable in the races of mankind. Among ancient Greek artists a deep nasal root was much esteemed. The line of the forehead was carried almost without a change in direction along the nasal bridge.

The instrument used in measuring was the sliding calipers with moveable arm reversed. The subject was told: "Shut your eyes." It was usually necessary to cant the plane of the graduated arm so as to avoid the eyeball. The measurement was made on each side of the nose and the mean of the two measurements taken as height of nasal root. Even so the error in measuring between landmarks is high; the curve of growth has to be much smoothed.

b. Results.

b′. Mass Statistics.

a. General.—It is hard to say when the nasal root first makes it appearance in the embryo; it is the most slowly developed part of the nasal bridge. Its existence depends on the arching of the nasal bones and connective tissue and skin that overlie them.

At the time when the procerus muscle and something that may be regarded as the beginning of the nasal bridge arise between the eyes [1] the root depth cannot be properly measured. Since the eyes and the inner eye angle are still somewhat laterally placed on the head, and the landmarks are moving, the earliest measurements would not be significant. But after the 2nd month the eyes have gained a more nearly frontal position and a nasal root depth of about 2 mm can be measured.* At birth the depth of the bridge at the nasal root is about 6.5 mm. After birth the

[1] Compare Keibel-Elze, Normentafeln des Menschen, Fig. 41 and Peter's (1913) Fig. 18. These show the profile of face of an embryo 20 mm long, approximately 7 weeks old.

* Cf. Pl. VIa.

depth of nasal root increases at the rate of 3 mm p.a. for a year or two (Fig. 28); but after about two years, at a time when the body as a whole has a reduced rate of growth,

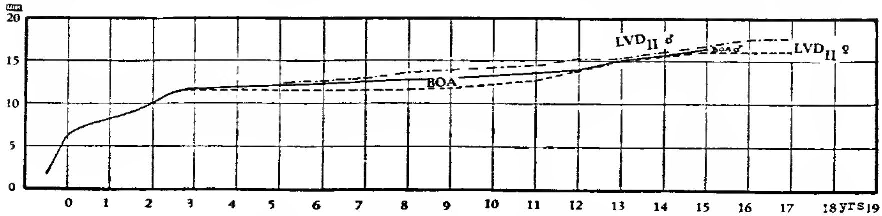

FIG. 28. Mass curves of growth of nasal root depth, embryo to maturity, combining data as in Fig. 1. At the later ages data are given both for BOA ♂, LVD$_{II}$ ♂ and ♀.

the rate is reduced for a time to about 0.5 mm p.a. (Pl. III, Fig. 1). At the time of the adolescent spurt the rate increases, and growth of the nasal root continues until 17 years and probably several years later, reaching, on the mean, about 18 mm in the case of males.

β. Sexual.—A differentiation of the sexes in respect to nasal root-depth can first be clearly made out at about the end of the second year (Fig. 28). Thereafter, while the nasal root-depth shows slow change for 6 years in the female, it increases 1.5 mm in the male, so that at 9 years the sexes are over 2 mm apart. Then in the female the nasal root increases in depth and at 13 years has reached the mean male depth also of 15 mm. After that the mean depth of nasal root, of the male, increases rapidly while that of the female grows very slowly. At 16 years the male has a 1.5 mm greater depth of the root. The approach of the mean nasal roots at about 13 reminds us again of the precocious spurt of growth of the general body of the female which tends to decussate the male curve of general body growth at about this age.

γ. Social.—Figure 29 shows mean values of nose bridge depth at root for 4 classes, all Nordic or U. S. stock. At age 12 the standard (BOA) boys have the deepest bridge, 14.0 mm; next below stands the LVD group at 13.5 mm;

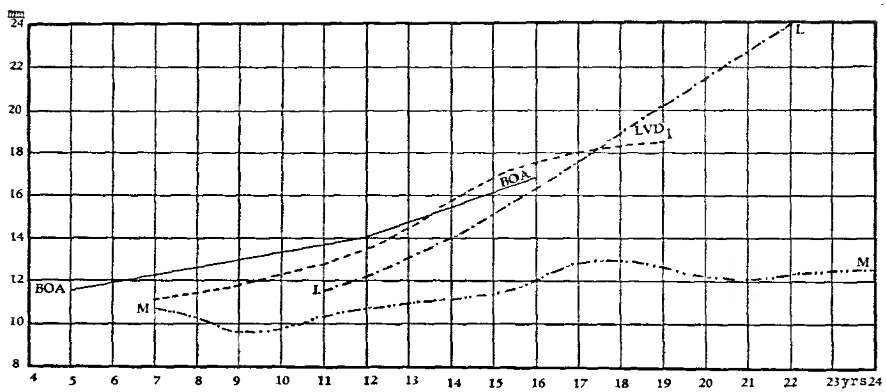

FIG. 29. Mass curves of growth of nasal root depth in four social groups: BOA, LVD_I, I and M. Nordic males only.

still lower, the idiot group, at 12.2 mm, and lowest the mongoloids with a root depth of 10.6 mm. This shoal root of the mongoloids is highly characteristic (Pl. IV, Fig. 7). On the one hand it is a persistence of an infantile character, since 10 mm root-bridge depth is characteristic of 2 years of age (Fig. 28). On the other hand, it is usually associated with an epicanthus, as in so many infants and many adult Chinese (Pls. IX and X).

Rather striking is the fact that after about 17 years the order of the first three groups becomes reversed so that at 20 years the idiot group comes to have much the greatest root depth.

δ. *Racial.*—Figure 30 gives the mean curve of growth of nasal root-depth in four racial groups of the LVD_{II} series, males. The U. S. and mixed (Nordic) group, beginning

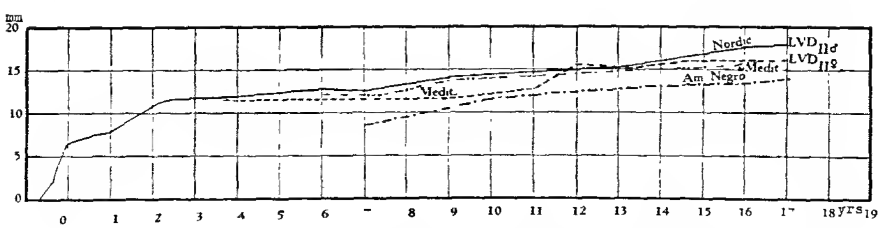

FIG. 30. Mass curves of growth of nasal root depth in three racial groups: Nordics, male and female (LVD_{II}), Mediterranean and Negro males.

at 6 years with 12.5 mm, increases at the rate of 0.4 mm p.a. until 14 years; from 14 to 15 years it increases 1.2 mm p.a.; for the next two years 0.3 mm p.a.

The mean Mediterranean group has from 10 years on a mean nasal root bridge shallower than that of the U. S. group by from 0.2 to 0.9 mm. The Jewish group has a still smaller nasal root, at least until puberty. The American Negro has a very shallow nasal root at 7 years—8.2 mm or less than two-thirds of the whites. This increases rapidly to 9 or 10 years (up to 12 mm) and then very slowly to 12 years. A spurt of mean growth from 12 to 14 years brings the mean depth to 13.3 mm. At 16 years the mean depth is 13.5 mm, as contrasted with the mean of 17.5 for U. S. and Nordics.

b″. Individual Data.

α. Comparative.—Figure 31 gives the curve of growth of root depth for 5 of the 9 boys shown in Pl. VII, taken

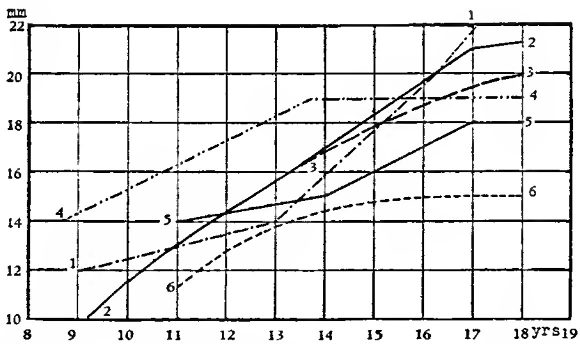

FIG. 31. Individual curves of growth of nasal root depth in six individuals of LVD$_I$ series, of all of whom photographs are shown on Pl. VII. 1, W.M., No. 8 (Nordic), Fig. 8; 2, J.C., No. 10 (Nordic), Fig. 14; 3, H.M., No. 43 (Nordic), Fig. 7; 4, J.B., No. 54 (Nordic), Fig. 6; 5, M.H., No. 2 (Nordic), Fig. 9; 6, T.V., No. 65 (Italian), Fig. 11.

at the age of about 18 or 19 years. It will be noted that there is commonly an increase in velocity of growth at 12 to 15 years; viz., in Nos. 43 (Fig. 7), 65 (Fig. 11), 2 (Fig. 9). Growth velocity is much reduced after 13 years in Nos.

54 (Fig. 6), and 65 (Fig. 11); after 15 years in No. 43 (Fig. 7); after 17 years in No. 2 (Fig. 9); and after 17½ years in No. 10 (Fig. 14).

The root depth shows a range of about 5 mm despite its low mean size.

Number 8 (Fig. 8) has the deepest root, 20.5 mm, and No. 65 (Fig. 11) has the shallowest, 17 mm. At 12 years they each had a root depth of about 13.5 mm. Consequently in them the root depth diverged. Again, No. 54 (Fig. 6) root depth at 18 years is mediocre at 18 mm, at 10 years had a deep root at 15 mm. In these cases the growth of the root is dissimilar.

On the other hand Nos. 10 (Pl. VII, Fig. 14) and No. 43 (Fig. 7) have parallel courses of growth of root depth.[1] On Pl. VIII the latest profile of each boy is traced; also (broken line) a profile 7 to 10 years younger.

Thus it appears that the difference in nasal root depth between any two grown boys is generally a trait that extends back to childhood. The difference is due to a difference in the way the two noses grow, and the difference often becomes accelerated as development proceeds.

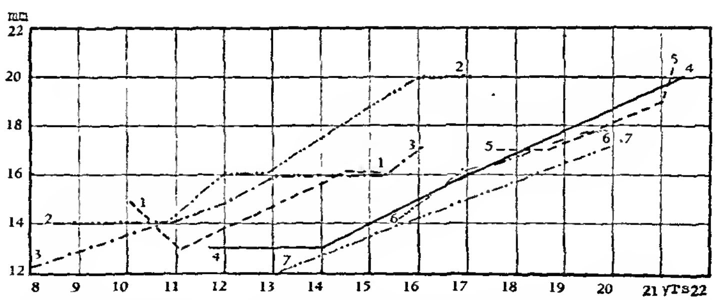

FIG. 32. Individual curves of growth of nasal root depth in 7 members of Mea. family (U. S. with Indian admixture). 1, George; 2, Reitha ♀; 3, Bertha ♀; 4, William; 5, Henry; 6, Marley ♀; 7, Nellie ♀.

β. Familial.—Figure 32 gives the greatly smoothed growth curves of seven members of the Mea. family. In

[1] In No. 43 (Fig. 7) the root depth is exaggerated in the photograph since it is not shown strictly in profile.

some cases the female nasal root is shallower (curves 6 and 7), in others (curves 2 and 3) deeper than in the 3 males (curves 1, 4 and 5). The boys' curves lie fairly close together, the girls' curves fall into two groups.

γ. *Twins.*—The nasal root bridge in the G. twins in the years in which it was measured showed the same dimensions within the limit of error in measuring, viz. 1 mm.

II. Ratios

1. *Development of the Nasal Index (Percentage Ratio of Nasal Width to Height)*

a. Definition.

This nasal ratio in the living is regarded as important because it is the only strictly nasal ratio that can be computed from skulls; and it is by skulls that we can trace human evolution. Unfortunately, however, there is not a close relation between nasal width and height as measured in the living and the corresponding dimensions of the *apertura piriformis* + nasal bones. In the living, the difficulty in finding precisely nasal height exaggerates the irregularities of the index.

b. Results.

b'. Mass Statistics.

α. *General.*—A generalized curve of change of the nasal index with age from about the fifth month of gestation to maturity is given in Fig. 33. The prenatal portion of this curve is based upon observations made by Dr. Adolf H. Schultz, confirmed by others of my own. The first three postpartum years are based on my observations at Babies Hospital. The remainder of the curves are based upon longitudinal observations made at Letchworth Village during 15 years.

The nasal index at the first appearance of a nose is around 115, width being greater than the height. At birth

the maximum is slightly below 100,[1] width being only slightly less than the height. Since the nasal height increases very rapidly after birth, while the width increases

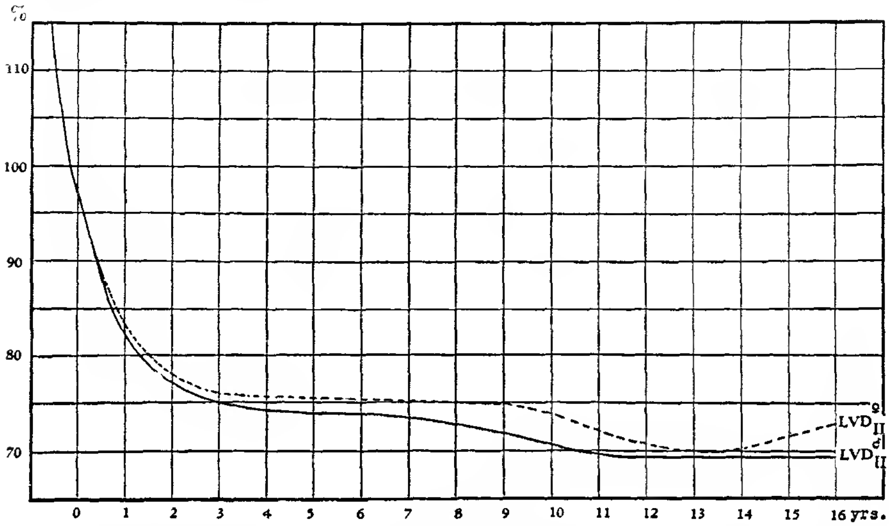

FIG. 33. Average age change of nasal index (percentage ratio of width to height), from embryo to maturity, combining data as in Fig. 1. Older children, high grade LVD_{II}, Nordic. The ordinates are percentages. Continuous line, male; broken line, female.

more slowly, as we have seen (Fig. 14) the nasal ratio falls rapidly to 3 years (when it is about 75) and thereafter it changes very little for about 4 or 5 years. At adolescence the ratio falls again to 70, as the slope of nasal height growth increases.

β. *Sexual.*—The curves of growth of the nasal indices of the two sexes begin visibly to diverge shortly after birth; and thenceforth the ratio is less in the male than the female. This is partly because the nasal height is greater in the male (Fig. 1) corresponding to his greater stature, while there is little difference in width, though the average width of the female nose is slightly the smaller (Fig. 14). Since there is a spurt in average nasal height (as in stat-

[1] Houzé (1888, p. 181) finds a mean nasal index of 104.7 for Belgian neonates, Blind (1890, p. 21) of 106.7 for Münchener neonates.

ure) in the female which soon flattens out again, the curve in girls is depressed so as nearly to meet the curve of male ratio at 13 to 14 years; but it soon departs from it. The female nose is much the lower and somewhat the narrower at all ages. The average nasal index of boys seems to change little after 12 years.

The curve of Fig. 33 agrees fairly well with the mean measurements of 25 children from Brussels at each age, as given by Martin ('28, p. 557), which run from 104.7 at birth to 69 at 21 years. Schwerz's ('10) ratio for boys at 15 years is only 61.5 as contrasted with my 69, which indicates that he placed the nasion higher than I. In Schwerz's series the nasal index of the girls is below that of boys between 6 and 10 years; is thereafter chiefly above until 16 years. Houzé ('88, p. 188) finds a mean nasal index for adult Flemish males of 67.7; females, 68.3; for Walloon males 70.0; females, 70.4.

γ. *Social.*—The curves of nasal indices of BOA (standard) and LVD$_I$ Nordic and U. S. run nearly parallel courses from about 74 at 6 years to 67 at 15 years (Fig. 34).

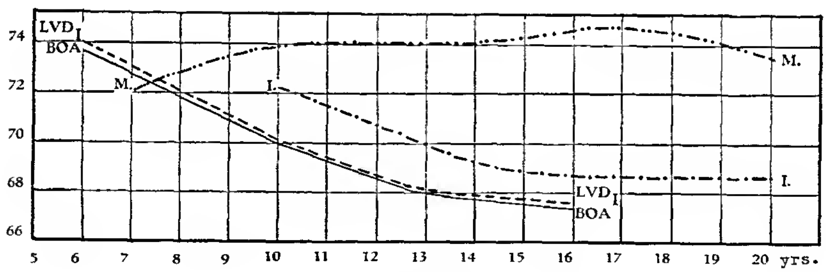

FIG. 34. Average age change of nasal index in four social groups: BOA, LVD$_I$, I, M.

The Idiot series lies always above the other two, by about 1 per cent. The nasal width in this series is fairly large (Fig. 15). The mongoloids have an exceptionally high index due to the small nasal height. (Pl. IV, Figs. 5, 7). Their nose is of the infantile type.

δ. *Racial.*—The U. S. and Nordic averages run from about 72 at 6 years to 67.5 at 15 years (Fig. 35). The

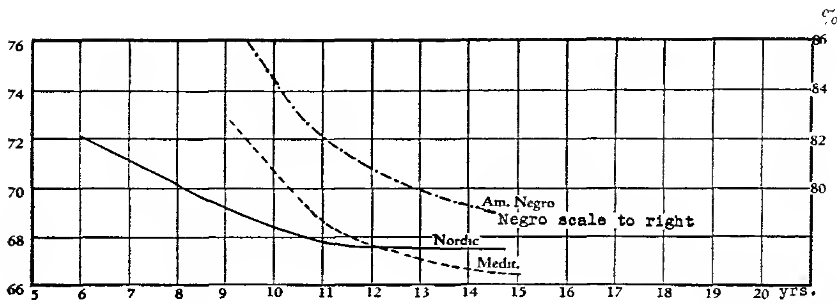

FIG. 35. Average age change of nasal index in three racial groups in LVD series: Nordic, Mediterranean, Negro.

Mediterraneans seem to have a slightly higher ratio before 12 years and a lower one later. The "Negro" (Negro-White) nose has a ratio about 14 points higher than the Nordic at all ages, owing to the great breadth and small height. Thus the index changes from 86 at 9½ years to 79 at 14½ years. The Batwa Negroes, as measured by Czekanowski, have a mean nasal index of 86.8; the Fans of 91.1 (Poutrin). One sees how the hybrid nose index of U. S. Negroes has diminished on the average half-way down to the Nordic level.

b″. Individual Data.

α. *Comparative.*—As one of the most variable of facial features the nasal index in its growth falls into different types in different individuals.

Type I. Decreasing index with age. This type is illustrated in Fig. 36. The curves run more or less nearly parallel courses except for No. 2. The decrease is rapid (1 to 2 points a year) to 11–13 years and slows up thereafter, sometimes to a constant ratio.

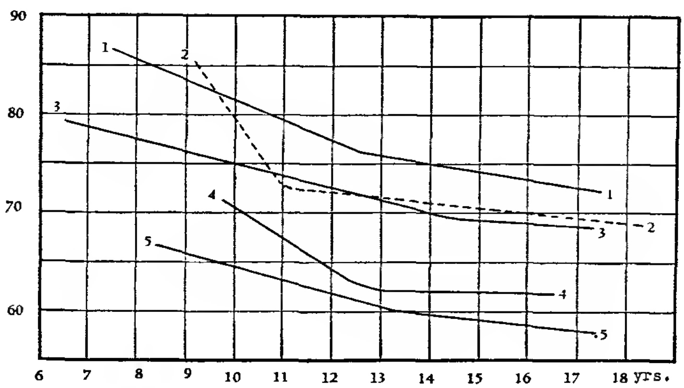

FIG. 36. Average age changes of nasal index (percentage ratio of width to height), decreasing with age, for five individuals of LVD$_I$ series, males. 1, G.T., No. 60 (Ital.); 2, T.V., No. 65 (Ital.); 3, D.F., No. 57 (Ital.); 4, R.C., No. 74 (Ital.); 5, E.H., No. 79 (Nordic).

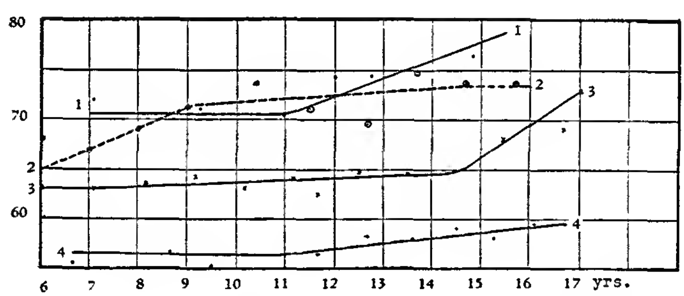

FIG. 37. Average age changes of nasal index (percentage ratio of width to height), increasing with age, for four high grade LVD$_I$ boys. 1, G.H., No. 82 (Nordic); 2, No. 83 (Nordic), brother of foregoing; 3, E.H., No. 1 (Nordic); 4, S.B., No. 55 (Nordic). Observations plotted.

Type II. Increasing with age (Figs. 37, 39). In this type we find individuals whose nasal index is increasing at 7 or 8 years when, on the average, the index is decreasing. The greater increase of course comes later at 11 to 15 years when nasal width on the average is increasing faster than nasal height.

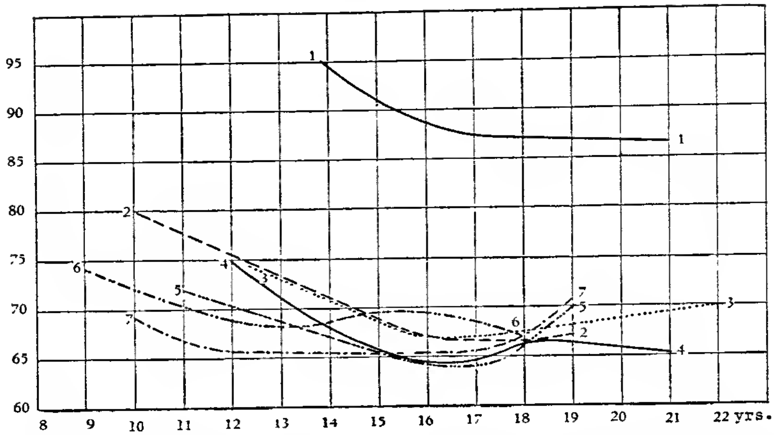

FIG. 38. Average age changes of percentage ratio of width to height, seven boys of the Idiot series. 1, R.H., No. 22 (Am. Negro); 2, S.C., No. 26 (Ital.); 3, J.C., No. 5 (Ital.); 4, R.N., No. 20 (Polish); 5, R.B., No. 11 (Jew); 6, W.E., No. 15 (U. S.); 7, L.C., No. 6 (Ital.).

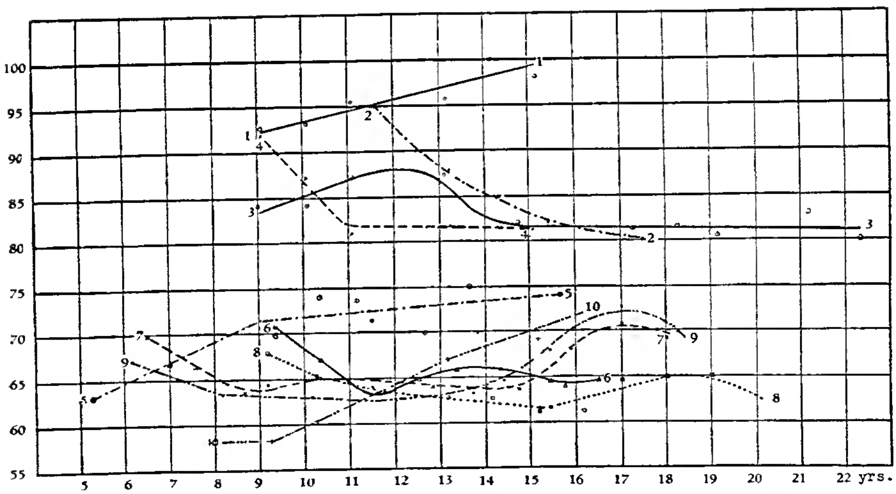

FIG. 39. Average age changes of nasal percentage ratio of index width to height of ten individuals of LVD$_I$ series. 1, C.H., No. 26 (Am. Negro); 2, F.B., No. 48 (Am. Negro); 3, C.D., No. 30 (Nordic); 4, E.T., No. 14 (Am. Negro); 5, C.H., No. 83 (Nordic); 6, G.M., No. 77 (Nordic); 7, J.B., No. 54 (Nordic); 8, H.F., No. 20 (Jew); 9, E.H., No. 1 (Nordic); 10, I.S., No. 93 (Polish). Males only.

Type III. The U-shaped type, concave above (Figs. 38, 39). This type agrees roughly with the mass average, as shown in Fig. 33. The bottom of the concavity occurs at different ages: 11½ to 16½.

Type IV. This is characterized by no marked trend in the index (Fig. 40, 5-7). This is the least common of the types.

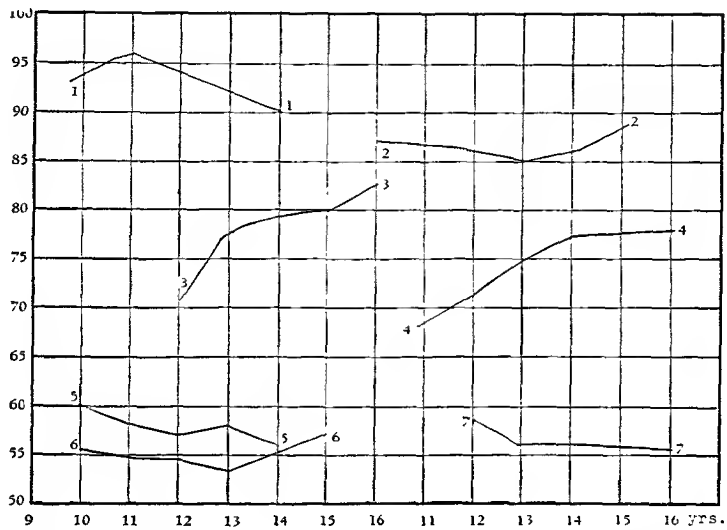

FIG. 40. Average age changes of percentage ratio of width to height for seven individuals of the LVD$_{II}$ series. 1, G.W. ♀ (Am. Negro); 2, L.B ♀ (Am. Negro); 3, M.H. ♀ (Nordic); 4, A.D. ♂ (Ital.); 5, C.A. ♀ (Nordic); 6, N.C. ♀ (Nordic); 7, M.F. ♂ (Jew).

Figure 40 gives curves of growth in nasal index for 4 girls and 3 boys. Among the girls No. 6, N.C. (Pl. VII, Fig. 2), is a Scandinavian with a low nasal index because of narrow but high nose. At 10 years the index was 55, at 14 years has, because of considerable increase in nasal width, increased to 57.

Number 5, C.A., of U. S. stock (Pl. IV, Figs. 1, 3), has a low nasal index because of both narrow and long nose. At 10 years the index was 60; it fell to 56 at 14 years, in consequence of rapid growth of nasal height.

Number 3, M.H., of Old American stock (Pl. IV, Figs. 2, 4), has a nasal index that is high for whites. Starting at 70.5 at 12 years it has risen to 82 at 16 years. The nasal height has meanwhile increased little, 44 to 45 mm, while the nasal width has increased from 31 to 37 mm.

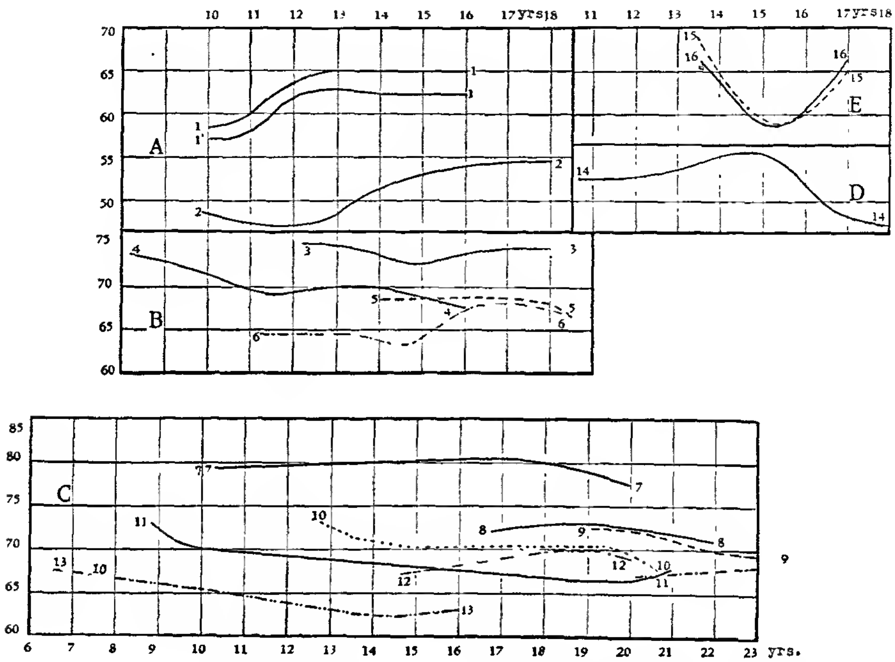

FIG. 41. Average changes of percentage ratio for individuals, of families and of special cases. (*A*) 1, Angelina G., 1, Mary G., identical twin sisters; 2, Joseph G., brother of the foregoing. (*B*) Sti. Family. 3, Lloyd S. ♂; 4, Myrtle S. ♀; 5, Claude S. ♂; 6, Edith S. ♀. (*C*) 7 to 13, members of the Mea. Family. 7, William M. ♂; 8, Henry M. ♂; 9, Leonard M. ♂; 10, Nellie M. ♀; 11, George M. ♂; 12, Marley M. ♀; 13, Bertha M. ♀. (*D*) 14, Microcephalic Italian male. (*E*) 15, 16, *D*, twins (Colored).

Number 1, G.W. (Fig. 40), is a negro x white cross. Her nasal index has fallen from 95.5 at 11 years to 90 at 14 years, since the nasal height has increased from 45 to 49 mm and nasal width from 43 to 44 mm.

Among the boys, No. 7, a Jew, has a low nasal index. This has fallen from 58.5 at 12 years to 55.5 at 16 years.

Nasal height has grown much faster (from 48 to 54 mm) than nasal width (from 28 to 30 mm).

Number 4, an Italian, has a high nasal index, increasing from 68.5 at 11 years to 78 at 16 years. The growth of his nose in width has been very great, from 30 to 35 mm, while nasal height has increased only about 2.5 mm.

Number 2 is an American negro hybrid whose nasal index has varied between 87 and 88.5. Nasal height and width have grown in nearly parallel fashion.

β. Familial (Fig. 41, *C*).—*Mea. Family.* This is one of the Village's large families. The nasal index ranges at 14 years from 62.5 (No. 13) to 80.5 (No. 7) at 17 years and 3 months. At around 21 years the surviving curves run close with less than 5 points difference between them. Nellie (No. 10) and Marley (No. 12) run very close together as do also Leonard (No. 9) and Henry (No. 8).

Figure 41, *B*.—*Sti. Family.* The nasal index ranges from 62.5 (No. 6) at 14½ years, to 74 (No. 3) at 12½ years. The curves of Myrtle (No. 4) and Claude (No. 5) and Edith (No. 6) at 16 years are close together (within 2 points).

γ. Special cases.—The G. twins. In these monozygotic twins because the height and width run along nearly identical paths the nasal indices are closely similar, differing by only 2 or 3 p.c., within the limits of error of measuring (Fig. *B*, page 196; Fig. 41, *A*, 1, 1').

The E. twins (curves 15, 16) have nasal indices that coincide for the most part to within 1 point, which is quite within the possible error of measurement.

Microcephalic. One microcephalic boy (*D*, curve 14) was measured during the course of 7½ years. In his case the nasal index was low, rising from 62.7 at 10½ years to 65.8 at 15 years and then diminishing again to around 58 at 17 to 18 years.

c. Discussion

In a series of young people of European origin or stock we have found nasal indices varying from about 56 to

82 per cent, thus running the gamut from leptorrhin (55.0 to 69.9 per cent) to among the highest of the mesorrhin (70.0 to 84.0 per cent). The lowest index from a male is of a Jew, the lowest from a female is of a Scandinavian. On the other hand, an Italian boy has one of the highest indices among the males. One of the highest indices among the white girls was found in one of English-German descent. From Martin's ('28, p. 551) table we find that Russian Jews have a low index (mean 61.5) and so do Norwegians, mean, 62 (Bryn, 1930). Few if any European stocks have a higher mean nasal index than 73.

It will be observed that the curve of the development of the nasal index is rarely a horizontal line or an oblique straight line. There is, as it were, a "struggle" between the height and width of the nose during development. Now one grows slowly, now the other. That is, the curves of nasal height and nasal width do not run parallel in the individual. Thus the working out of the different genes is not at a steady, still less at a parallel, rate. Nasal height is determined by growth of the bones of the face in general, of the nasal bones, and of the nasal cartilages; and the growths of these different parts is not closely synchronous nor closely interlocked. Notwithstanding, the end result is usually a family resemblance in nose form. But in the case of identical twins the growth of the different parts that contribute to nose form run strictly parallel or even identical courses (see Fig. *B*, p. 196). So while the growths of the different parts may not be uniform they are far from being haphazard or uncontrolled.

2. *Development of the Nostril-Surface-Plane Index* (*Percentage Ratio of Nasal Depth to Width*)

a. Definition.

The depth of the nose is taken as the horizontal distance from the pronasion (nasal apex) to the nasal wing sulcus, measured parallel to the sagittal plane. This agrees with

Martin's ('28, p. 188): "22a, Tiefe der Nase"; not with his No. 22, which I call nasal salient.

This ratio of depth to width (between alæ) is one which appears most obvious when the nostrils of the outer nose are looked into. The great range in Homo sapiens is appreciated by comparing the narrow nose of the Norwegian, averaging 35 mm in the male and 33 mm in the female (Schreiner, '30, p. 36) with the wide nose of the West African Negro, averaging 41.6 mm in the male (Weninger, '27, p. 84). The range of width of the West African nose was found to be 37–47 mm.

b. Results.

b′. Mass Statistics.

a. General.—Figure 42 gives a mass growth curve of the nasal depth/nasal-width ratio from before birth to ma-

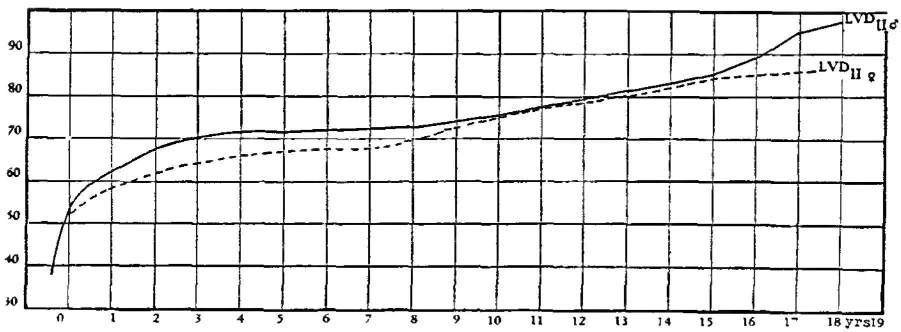

FIG. 42. Average age changes of percentage ratio nasal depth to nasal width, embryo to maturity, combining data as in Fig. 1. 6 to 19 years, LVD_{II} series. Continuous line, males; broken line, females. Ordinates are percentages.

turity. Owing to the shallowness of the nose when first formed the index at that time is extremely low, 38. At birth it has risen to about 53. The rate of change of the index slows down after 1 year to 5 points, then to 3 points per year, and after 3 years undergoes almost no change until, shortly before adolescence, an increase of slope occurs to 2 points per year. After 15 years in boys the slope increases for a time to 4 or 5 points per year and then growth

of this index gradually diminishes to zero. Since nasal width increases slowly as compared with depth, the changes in the ratio are mostly due to changes in the latter dimension.

β. Sexual.—A difference between the sexes in this ratio shows itself at, and probably arises before, birth (Fig. 42). The female ratio is the smaller by about 5 points until about 8 years of age. Thereafter it reaches the male ratio and continues near it until age 15. Thenceforth the spurt in the curve of male ratio (increasing depth associated with total body increase) carries it away from the female ratio, since the latter has had its spurt about 5 years earlier. The female has, at 18 years, a shallower nose than the male.

γ. Social.—While the standard BOA boys at 8 years have a mean nostril-plane index of 77, a little below that

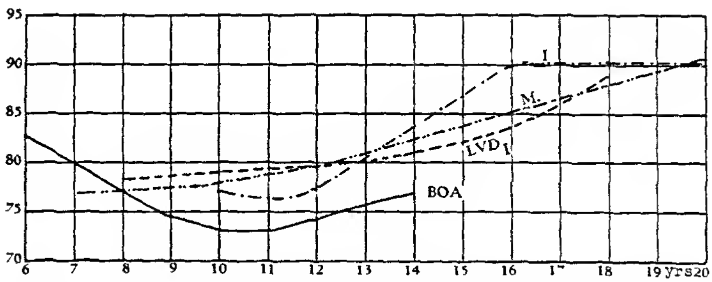

FIG. 43. Average change with age of the percentage ratio of mean nasal depth to nasal width for four social classes, chiefly Nordic males. BOA, LVD$_{I}$, I, M.

of the LVD$_I$ series (Fig. 43), the index falls to 73 at 10 to 11 years. It then rises slightly as depth increases rapidly (Fig. 7). Even so this group falls below all the others in this nasal index; signifying a relatively less deep nose. The indices of the three feebleminded series are constantly above standard and, after adolescence, the boys of the I series have the relatively deepest noses.

δ. Racial.—The changes with age of the nostril-plane index for our racial material is shown in Fig. 44. The

nostril-surface indices of the Nordic and Mediterranean males increase in more or less parallel fashion from about 72.5 to 74 at 5 years to 92 to 94 at 18 years. In the male

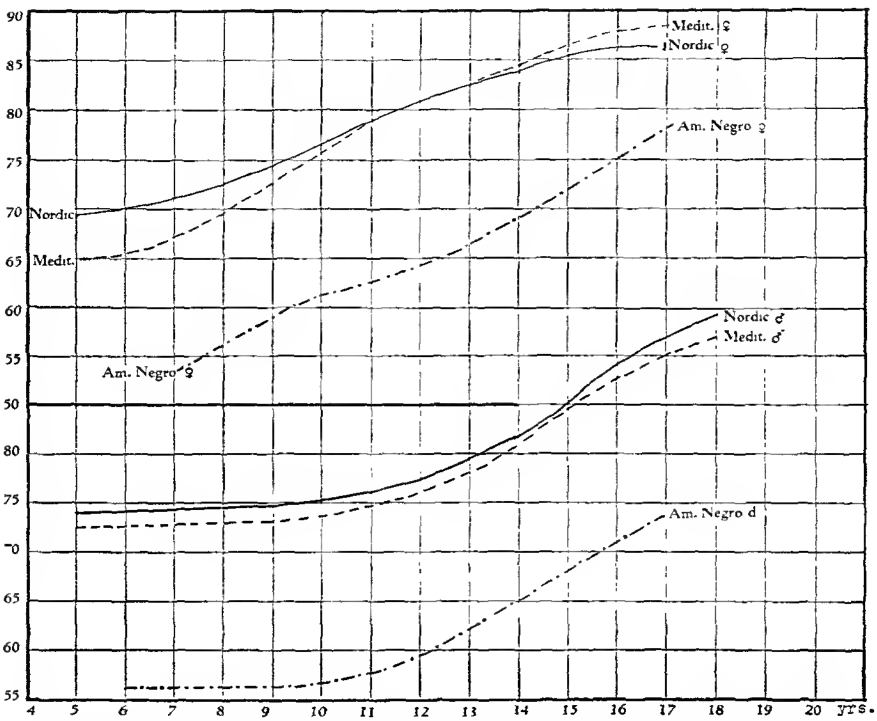

FIG. 44. Average age changes of the percentage ratio of nasal depth to nasal width, for three races LVD$_{II}$ series. Females above, males below.

the ratio undergoes slight changes from 5 to 10 years; then increases at the rate of about 1.5 to 2.5 points p.a. until at 16 years the rate of change begins to decrease. The Nordic and Mediterranean curves come close together at 14½ years, but do not decussate.

In girls the curve of the index starts, at 5 years, lower than in boys (at about 67 instead of 73). The Nordic ratio is at 5 years 5 points higher than the Mediterranean, but the curve of the latter advances at a more rapid rate at 8 years (3 points p.a. vs. 1.5 points p.a.) and the two curves

decussate at 12 years. At 17 years the Mediterranean ratio is 2 points higher than the Nordic. This result indicates that the nose of the Mediterranean group, on account of its narrowness (see Fig. 16) without corresponding shallowness in the female gains a slight preponderating nostril surface index during puberty.

In colored children the nostril-plane index lies very far below that of the whites by reason of the great width of the nose. In the male the ratio starts at about 56 at 6 years and rises to 74 at 17 years. During adolescence and early puberty it advances at the rate of 3 points p.a. Among the colored girls (Fig. 44) the curve, stating at 53.5, is already rising at 7 years and proceeds in nearly straight line fashion to 78 at 17 years, an average rate of increase of 2.2 points p.a.

b″. Individual Data.

α. *Comparative.*—On Figs. 45–47 are reproduced the curves of development of a number of white individuals of the LVDI and LVDII series. In general they fall into three types:

Type I. Straight line advance from childhood to maturity. This is not the commonest, but it is the simplest

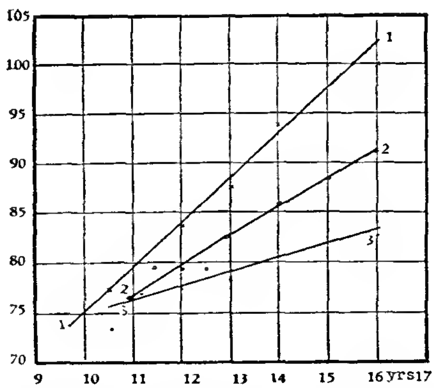

FIG. 45. Average age changes of the percentage ratio of nasal depth to nasal width for three individuals. LVD$_{II}$ series. 1, M.J., No. 55 (Nordic); 2, G.R., No. 88 (Nordic): 3, J., No. 72 (Italian). Observations plotted.

type. It is represented by the cases of Melvin J., George R., and Jordan M. (Fig. 45). In such cases the depth-producing and width-producing factors keep an even step. That is, the growth of the wing cartilages in depth and width is constantly at the same rate.

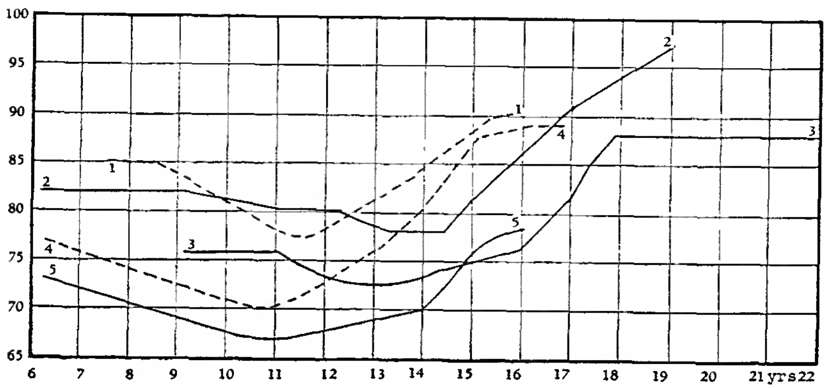

FIG. 46. Average age changes of the percentage ratio of nasal depth to nasal width for five boys of type 2 from Letchworth Village. 1, J.B., No. 54, Pl. VII, Fig. 6; 2, E.H., No. 1; 3, T.V., No. 65 (Italian), Fig. 11; 4, V.E., No. 59 (Nordic); 5, E.C., No. 58 (Nordic).

Type II is the commonest (Fig. 46). It is the U-type; during juvenility the ratio changes slightly or trends slowly downward. This is represented by the level part of the mass curve. A minimum of 67 to 78 is reached by different individuals at various ages from 11 to 14, followed by a fairly sharp rise. This minimum is not always disclosed in the mass curves just because it occurs at different ages; but the mass curves have, at these ages, a low gradient. The sharp rise in the mass curve at 15 to 17 years reflects the steep upward gradient of this type. This type of growth means that during childhood the alar cartilages are growing in width as fast as or faster than in depth; the nostrils are maintaining their shape or getting wider. During adolescence a change occurs; the growth of the alar cartilages in width is less than that in depth; the nostrils are becoming narrower. This type seems not to be

associated with any particular white racial stock; it occurs in my records chiefly in old U. S. stock, but also in Italians.

Type III. In this type there is no great change in pro-

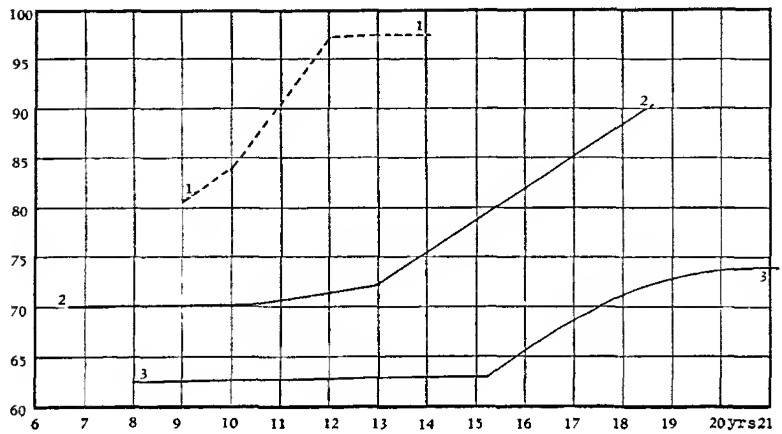

FIG. 47. Average age changes of the percentage ratio of nasal depth to nasal width for three individuals of Letchworth Village. 1, M.B., No. 8 (Italian); 2, D.F., No. 54 (Italian); 3, C.D., No. 30 (Nordic).

portions of the nose during juvenility and early adolescence; but in the middle or latter part of adolescence an abrupt increase of the upward slope of the ratio curve occurs, indicating that depth is now increasing rapidly as compared with width (Fig. 47). This is reflected in the mass curve by the steep gradient from 15 to 17 years.

The colored children show a developmental series of indices lying much lower than that of the whites. Some of these are given in Fig. 48. In general these show an approximately linear advance starting, at 10 years, at anywhere from 45 to 67. In some cases the advance shows a curve that is concave toward the base. The 13 year depression which is so prominent in Type II is missing from the colored children considered in my study.

β. Familial.—Figure 49 gives curves of nasal depth-width proportions of 4 boys in one family (Hic.). The curves of the nostril-plane index fall into two groups of

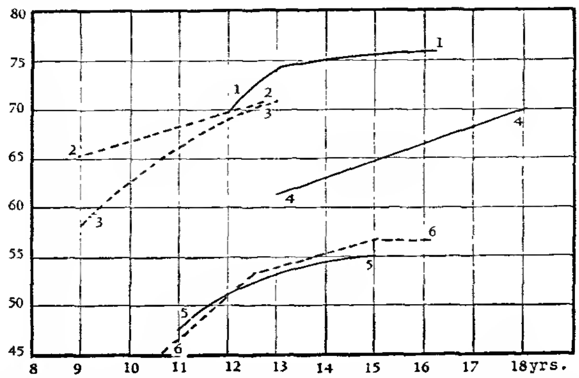

FIG. 48. Average age changes of the percentage ratio of index of nasal depth to nasal width for six colored children. 1, L.B., No. 17 ♂; 2, R.B., No. 120 ♀; 3, O.D., No. 121 ♀; 4, G.W., No. 111 ♀; 5, L.B., No. 19 ♂; 6, G.W., No. 109 ♀.

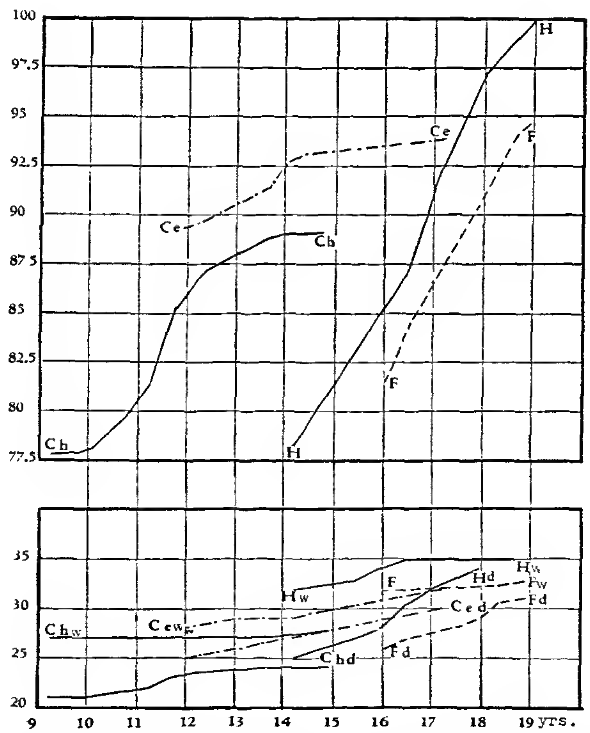

FIG. 49. Average age changes of the percentage ratio of nasal depth to nasal width for four boys of the H. family. Above, the ratios; below, the corresponding nasal widths (w) and nasal depths (d) are given for the four boys. Note that the ratios fall into two groups.

two boys each. The left-hand group is characterized by early cessation of increase in the index. This means that the growth curves of depth and width come to lie nearly parallel toward the end of the period of observation. The right-hand pair are characterized by a rapid ascent still persisting at an age 3 or more years older than in the first pair. This means that the curves of depth and width are rapidly approaching at 16–19 years and hence the ratio is approximating unity, *i.e.*, 100 per cent.

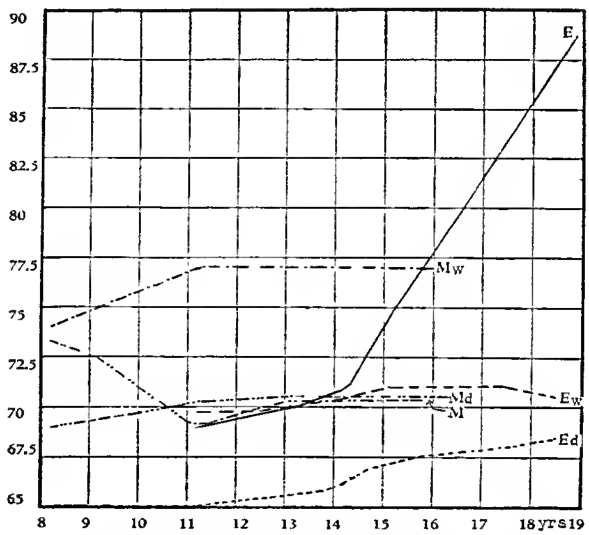

FIG. 50. Average age changes of the percentage ratio of nasal depth to nasal width for two sisters (E, M) of Sti. family. Below are given for the same girls the growth of nasal width (w) and nasal depth (d). Scale of w and d not given.

Figures 50 and 51 give the curves of growth of two girls and two boys all in the same fraternity. The curves of the girls are of two types: in one, the ratio ceases to increase after 14 years, because of cessation of growth of nasal depth; in the other it still ascends rapidly till after 18 years owing to the increasing depth of the nose.

The brothers (Fig. 51) both belong to the persistently increasing type, so that one of them has gained an index

of 100 per cent. This is due chiefly to the continued growth of the nasal depth.

Figures 52 and 53 show respectively 3 brothers and 4 sisters of one large family. The curves of ratios in Fig.

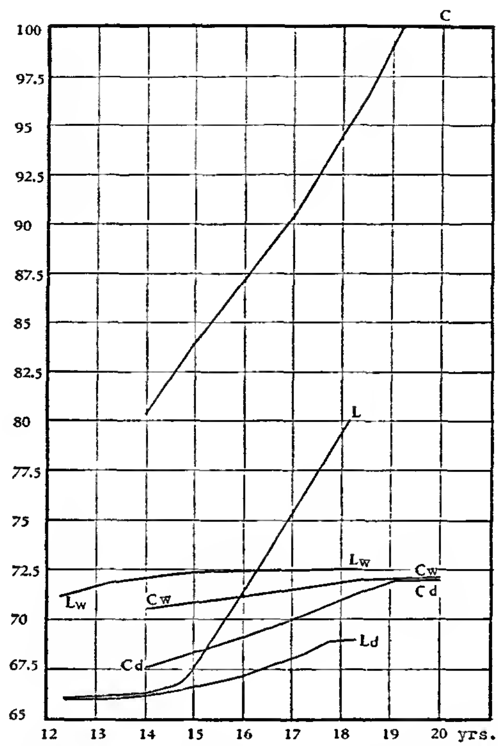

FIG. 51. Average age changes of the percentage ratio of nasal depth to nasal width for two brothers (C, L) in the Sti. family, siblings of girls in Fig. 50. Below are given the developmental curves of width (w) and depth (d) for these two boys. Scale of w and d not given.

52 are of two types: G. and W. rise fairly late in adolescence, or even later. H. has a diminishing course due to the faster growth of width than of depth. The girls (Fig. 53) also fall into the same two groups; namely, one with increasing ratio (increasing depth) and three with decreasing ratio (increasing nasal width). In both of the foregoing cases we have evidence of segregation of forms of growth in nasal dimensions.

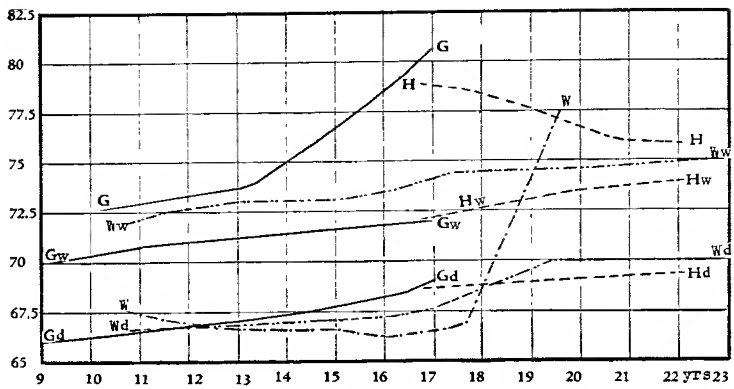

FIG. 52. Average age changes of the percentage ratio of nose depth to nose width for three boys of Mea. family, George, Henry and William, together with the developmental curves of nasal width (w) and nasal depth (d) of each. Scale of w and d not given.

It should be added that in boys in whom the nose has been measured to 20 years of age, or over, the nostril-plane index tends to become fixed or nearly so. The question of changes in nasal proportions in the third or later decade can not be answered from the data that we have available.

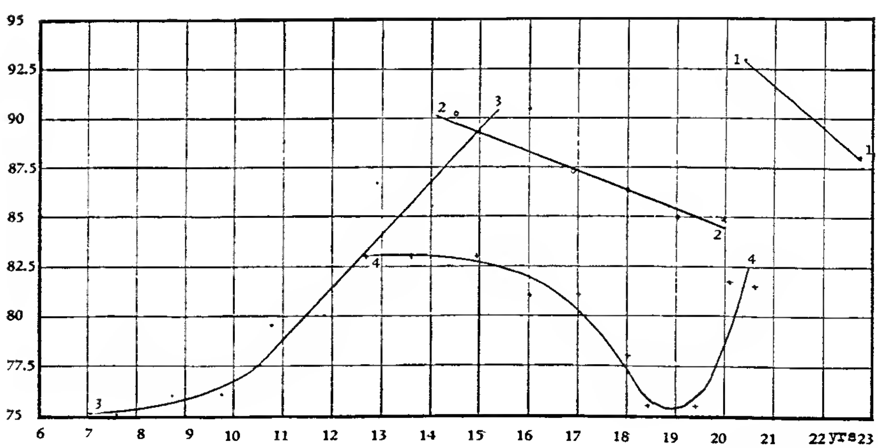

FIG. 53. Average age changes of the percentage ratio of nasal depth to nasal width for four sisters of Mea. family, siblings of boys in Fig. 52. 1, Mildred; 2, Marley; 3, Bertha; 4, Nellie. Observations plotted.

3. *Development of Relative Nasal Width (Percentage Ratio of Nasal Width to Bizygomatic Width)*

a. Mass Statistics.

α. *General.*—The first trace of the nose in the embryo is a pair of olfactory fields that arise laterally on the head (Pl. VI, 1). Eventually these come to lie close together in front of the face in the position of the nostrils. Nevertheless, so extensive is the growth of the head in width (as measured by the bizygomatic width) that absolutely these

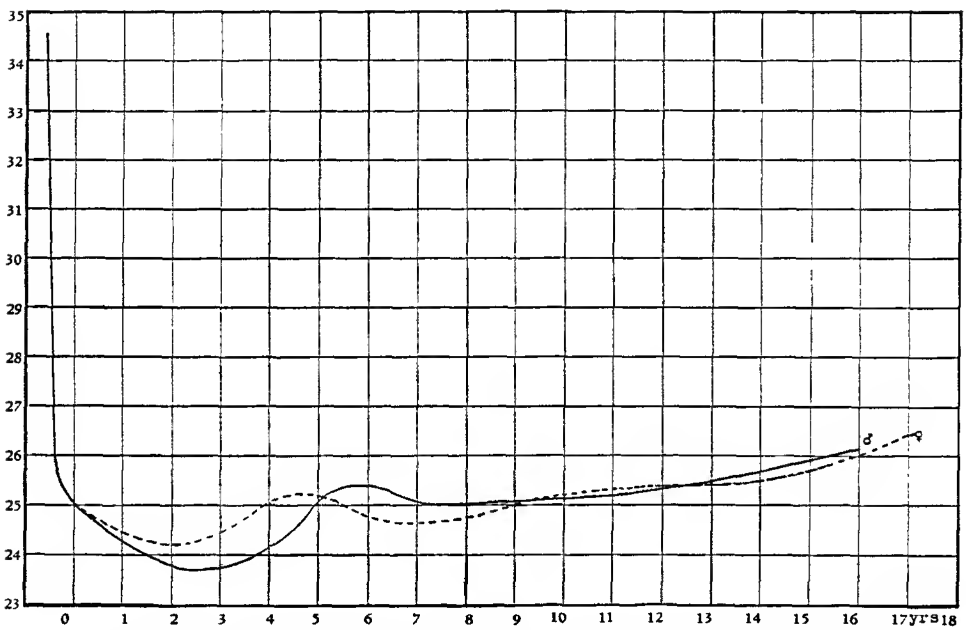

FIG. 54. Average age changes of the percentage ratio of nasal width to bizygomatic width, embryo to maturity, combining data as in Fig. 1. From 6 to 17 years BOA ♂ and ♀. Ordinates are percentages.

areas separate in the embryo, after the nostrils have come to lie 0.5 mm apart as we have already seen (p. 183). We have here to consider the growth of width of the nose in relation to that of the face as measured by the bizygomatic width.

β. *Sexual.*—Figure 54 shows the average growth curve

of this index. The prenatal portion is based chiefly on Schultz's ('20, p. 180) work checked by measurements made by me on fetal material at the Embryological Laboratory of the Carnegie Institution. The post-natal portion is based on Nordic babies from the Babies Hospital (New York) and on children from Brooklyn Orphan Asylum and Letchworth Village. In the prenatal series no distinction of sex is made, but from birth on sex is important.

The curves show that at 10 weeks of gestation the relative nasal width index is 34.5, *i.e.* the distance between the alæ of the nose is over one-third of the width of the face. During the next 3 months the alæ approach (relatively) with great rapidity owing, as we have seen (p. 183), to the rapidly widening face, although (p. 208) the nasal width is increasing. At or around birth the ratio is 25.[1] Except for the probable fall to below 24 at or about 2 years the ratio of 25 holds about constant to 8 years and then begins its rise to over 26 at 17 years. This rise is largely due to an increase of mean nasal width from 30 to 35 mm during these 10 years (Fig. 14).

The comparison of the curves of the sexes must be made with caution, since the differences are not great and only small numbers were measured. It seems probable that from birth to 5 years the relative nasal width of the female is the greater. From 5 to 9½ years it is less. From 10 to 12½ it is about the same as the male; thereafter it is less again. The rapid rise of the female curve from 7 to 11 is correlated with the rapid general increase of the body at this time; just as the rapid increase of the male index at 12 to 14 is associated with the male spurt of growth at this time.

It appears, however, that the spurt of growth affects the nose (and face) more in vertical than in transverse fashion.

γ. *Social.*—Figure 55 shows curves of ratio change with

[1] Blind (1890, p. 23) finds for neonates of München a relative nasal width with modes at 25 and 28.

age for U. S. and Nordic males of four social groups; BOA, high grade feeble-minded, imbeciles and mongoloids. In all cases the curves descend slightly from childhood into

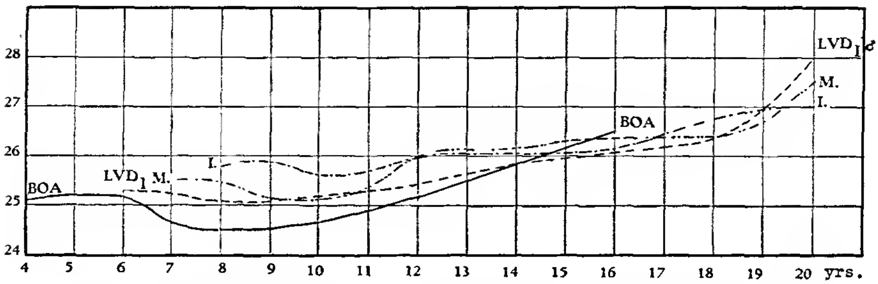

FIG. 55. Average change with age of the percentage ratio of nasal width to bizygomatic, in four social groups, Nordic males. BOA, LVD_I I, M.

juvenility, then rise steadily to puberty and sometimes increase rapidly after 18 years. The latter increase seems to be due to a failure to increase of the bizygomatic dimension after 18 years, while nasal width grows.

Comparing the four groups at 13 years it appears that the ratio increases in the order of the decreasing intelligence of the group. Now, nasal width is directly proportional to intelligence (and general bodily development); and the same is true of bizygomatic breadth. But whereas the range of mean nose width, in our 4 groups, is at 12 years 2.5 mm that of mean bizygomatic is 10 mm. Since the mongoloids have a very small bizygomatic width, the relative width ratio is in them large; and since the BOA's have a very large bizygomatic width the relative nose width ratio is in them small. At 15 years the different relative nasal width ratios are clustered close about 26. This is in accord with the finding that the bizygomatic widths are clustered close around 125, while the nasal widths of all four groups are close together (as always) and, at 15 years, around 32.5. And $32.5 \div 125$ is 26 per cent. It may be added that after age 16 the order of the curves becomes somewhat mixed, whether due to small samples, or to physical differences cannot now be determined.

δ. *Racial.*—As Fig. 56 shows, the two white groups have a relative nasal width index which fluctuates around 26, sinking, at 11 years, to below 26 and rising, in the Nordics,

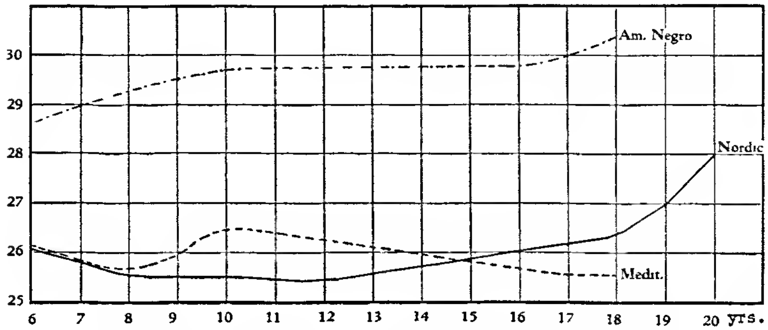

FIG. 56. Average change with age of the percentage ratio of nasal width to bizygomatic width for three racial groups, LVD males.

at 20 years to 28. The Mediterranean nose has a larger index than the Nordic at 9 to 13 years, due to the fact that the Jewish element of that mixed group has the highest index of all the groups at those ages. The mean nasal index in the Mediterranean group falls below the Nordic mean after age 15.

The colored group stands quite by itself with an index of 29 to 30. This is chiefly due to the great width of the nose of the Negro. The Negro's bizygomatic is also wide, about 2.5 per cent wider than the Whites'; but since the nasal width is about 15 per cent greater in the Negro than in the White the 13 per cent excess of the Negro is accounted for.

b. Individual Data.

α. *Comparative.*—The changes in the relative nasal width index tend, in general, to increase from 8 to 20 years; yet usually a period of fluctuating ratio between these extremes is found. Altogether three types may be distinguished.

1. *The level type* (Fig. 57). This is rare. In no case is the curve strictly level; the fluctuations are slight. This is illustrated by curve 2, No. 59 (V.E.), and curve 1, No. 28 (F.C.), both of the Nordic racial stock.

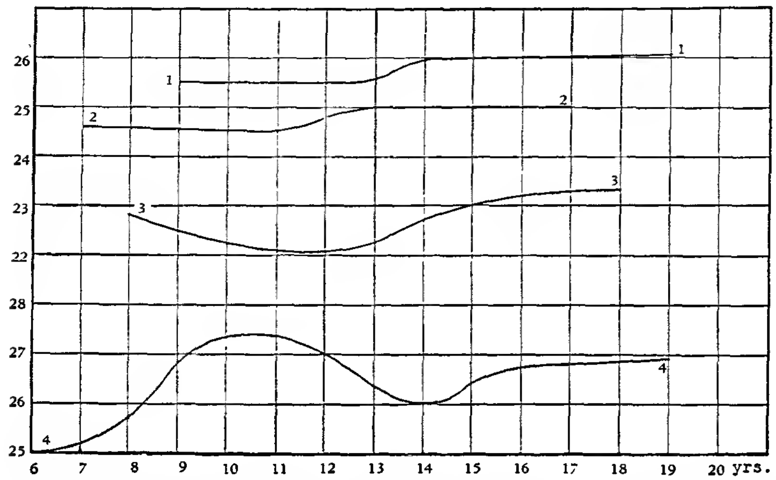

FIG. 57. Change with age of the average percentage ratio of nasal width to bizygomatic width, four boys of U. S. Nordic stock. Horizontal (approximately) type. 1, F.C., No. 28; 2, V.E., No. 59; 3, S.B., No. 55; 4, E.H., No. 1.

2. *The increasing type* (Fig. 58). This is seen in curve 4, No. 7 (A.M.), also in curve 2, No. 82 (G.H.), and curve 5, No. 5 (M.H.). These are all boys of northern or central European stock who attained a good size when full grown. Into this type also falls the case of R.H. colored (Fig. 59, curve 1).

3. *The wavy type.* This is seen typically in curve 4 of Fig. 57, No. 1 (E.H.), and curve 3, No. 55 (S.B.), of Fig. 57; also curve 3, No. 110 (R.O.), and curve 1, No. 81 (R.B.), of Fig. 58. Also curve 4, No. 67 (M.L.), and curve 6, No. 20 (H.F.), of Fig. 59. Evidently in these cases there are periods of relatively rapid and then of relatively slow growth of one or both of the dimensions involved in this

ratio. That is, in some cases the normal proportions of the nose are not preserved throughout childhood and juvenility.

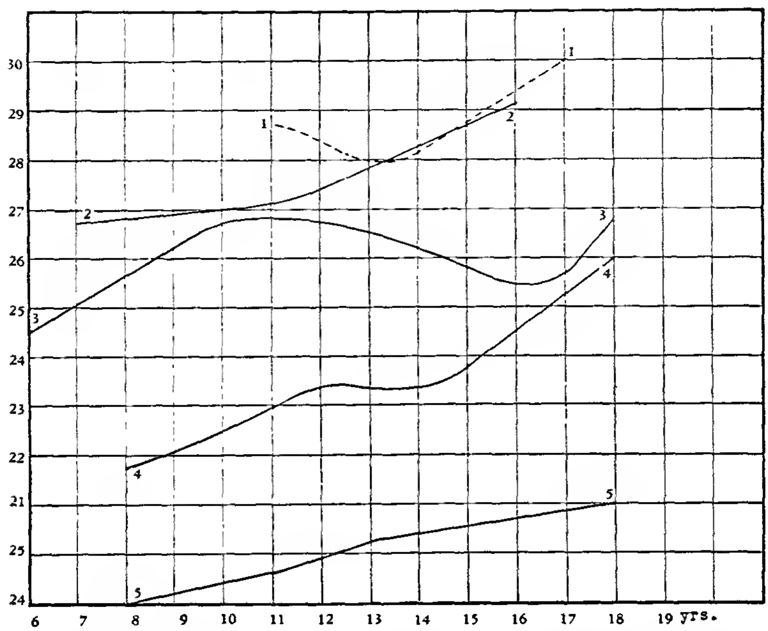

FIG. 58. Change with age of the average percentage ratio nasal width to bizygomatic width, individual males, LVD$_I$ U. S. Nordic stock. Increasing type. 1, R.B., No. 81; 2, G.H., No. 82; 3, R.O., No. 110; 4, A.M., No. 7; 5, M.H., No. 2. (Note changed scale.)

One notes also the great variation in this ratio in different white children. In Fig. 58 we have one curve running from 21.8 at 8 years to 25.9 at 18 years; another from 24.6 to 26.8; another from 26.8 to 29.1 and still another from 28.7 at 11 years to 30. Thus the nose width varies from about 20 per cent to 30 per cent of the width of the face. In colored children (Fig. 59, curves 1 to 3) the ratios run from 29.1 at 9 years to 33.7 at 18 years, or about a third of the facial width.

β. Familial.—Figure 60 gives sets of curves for representatives of three families. At the bottom are the curves of the pair of identical G. twins, whose ratios hover about 22¾. Above are curves of two brothers, centered about 26. At the top, curves of three brothers of a colored family

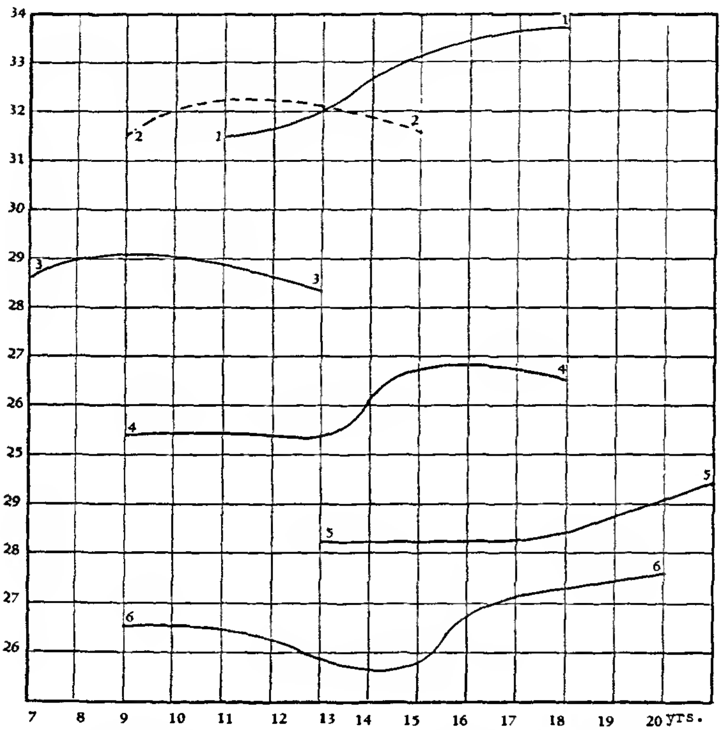

FIG. 59. Change with age of the average percentage ratio of nasal width to bizygomatic width. Individual males, LVD$_I$, Negro and Hebrew. 1, R. H., No. 80, Negro; 2, C.H., No. 26, Negro; 3, H.T., No. 3, Negro; 4, M.L., No. 67, Hebrew; 5, F.C., No. 45, Hebrew; 6, H.F., No. 20, Hebrew.

whose indices are centered about 29. There are clearly familial as well as racial differences in the growth of the relative nasal index.

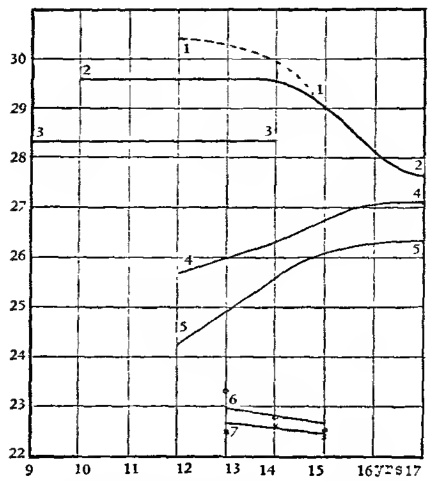

FIG. 60. Change with age of the average percentage ratio of nose width to bizygomatic width. Family groups. But. family, Negro, three brothers. 1, E.B., No. 51; 2, F.B., No. 48; 3, M.B., No. 21. Two brothers of Ful. family, Nordics; 4, J.F., No. 112; 5, L.F., No. 113. The G. twins, Italian girls; 6, M.G., 7, A.G.

4. *Development of Relative Nasal Salient (Percentage Ratio of Nasal Salient to Depth)*

a. Definition.

The relative nasal salient is the salience of the nose (tip to lip) in relation to nasal depth (tip to cheek sulcus). This ratio represents the proportional flexibility of the nose. It reaches its highest point among Primates in the genus Nasalis.

b. Results.

b′. Mass Statistics.

α. *General.*—Figure 61 shows the average general progressive change in this ratio from embryo to maturity. The index begins at once with the rising of the nasal papilla at 7 or 8 weeks. The ratio is at first about 30 per cent, rises rapidly to 70 per cent at birth (and even to 71 per cent in early infancy). These data are derived from the Carnegie

Embryological Laboratory and Babies Hospital. After one year the ratio falls more or less regularly to about 55 per cent at maturity.

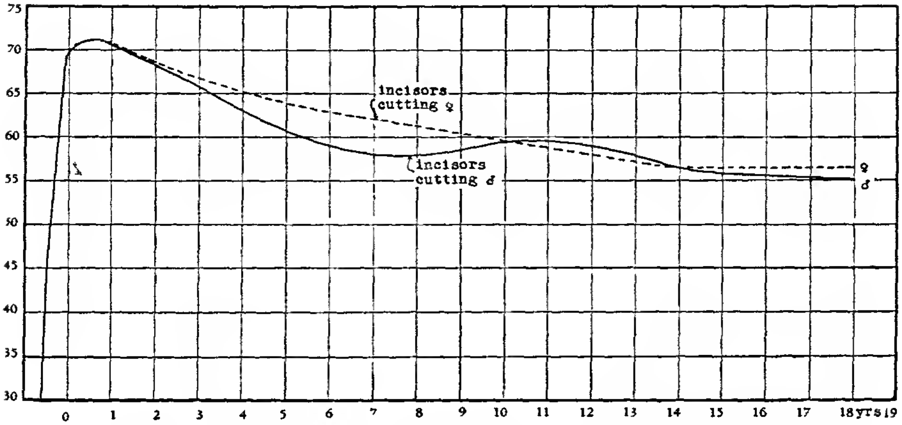

FIG. 61. Average age changes of the percentage ratio of nasal salient to depth, embryo to maturity, combining data as in Fig. 1. From 7 to 19 years LVD_I and LVD_{II} series. Ordinates are percentages.

Thus the mass curve of growth of this index is of the tent type, one branch rising rapidly to a peak at or near birth; the other branch falling away more gradually. In other words, while the nasal sulcus and alæ are laid down early the median nasal process is slow in appearing (Pl. VI). It is eventually made manifest through a combination of a tranverse groove between the eyes determining the nasal root and the formation of a hillock over the growing nasal cartilages, which come to enfold the nostrils and carry them outward perpendicularly to the face. This process is really the new feature in the evolution of the human face. Having made a late start the process develops very rapidly on top of the still feeble bones of the face. But after birth the maxilla develops rapidly in connection with tooth formation and the sulcus becomes left behind while the upper lip is pushed forward. Consequently, nasal depth increases faster than nasal salience and the ratio between the two decreases.

β. Sexual.—Figure 61 shows the curves of development of the relative nasal salient of the two sexes. In general the curves proceed closely together, the male index being probably slightly less than the female from 3 to 10 years. The ♂ ratio falls pretty steadily from 1 to 8 years [1] and after that rises slightly, possibly due to a further adjustment of the maxilla as the permanent dentition is fully formed. Indeed, there is some evidence from the individual records that the ratio may decrease in males at 13–14 years, perhaps in connection with thickening of the upper lip as the hair follicles enlarge. The female ratio decreases quite regularly from birth on.

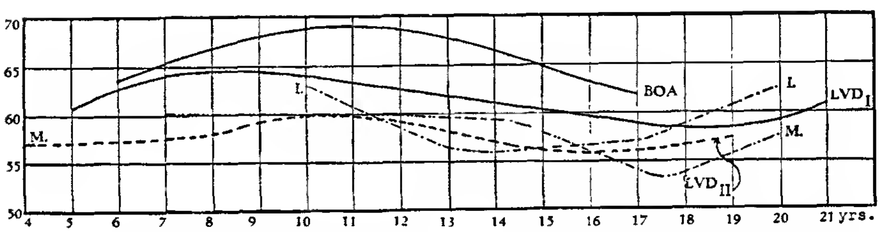

FIG. 62. Change with age of the average percentage ratio of nasal salient to nasal depth, five social groups, males, Nordics. BOA, LVD_I, LVD_{II}, I and M.

γ. Social.—Figure 62 based on U. S. and Nordic males, reveals the fact that the relative nasal salient is greatest in the standard (BOA) boys, less in the medium grade (LVD_I) boys and least in the higher grade (LVD_{II}) boys. It is unexpected that the LVD_{II} series is not intermediate between the BOA and LVD_I series. Greater numbers might give a different order.

In all cases the age curve of the indices rises to the beginning of adolescence, falls to the beginning of puberty and appears thereafter to rise slightly.

1 Perhaps because the salient growth is retarded because of the thickening of the maxilla, as the incisor teeth develop. *Cf.* Adams (1917, p. 564) who attributes the increased growth of the basion-naso spinal line at 4 to 10 years to development of the permanent molars; so also Hellman, 1927.

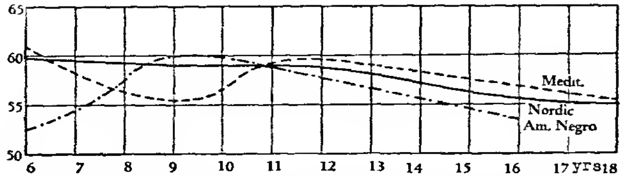

FIG. 63. Change with age of the average percentage ratio of nasal salient to nasal depth. Three racial groups LVD$_{I}$ series, males.

δ. *Racial.*—Figure 63 shows that the ratio is not very different between Mediterraneans and Nordics. The colored children had a slightly smaller ratio after 11 years than the whites, despite a smaller average nasal depth.

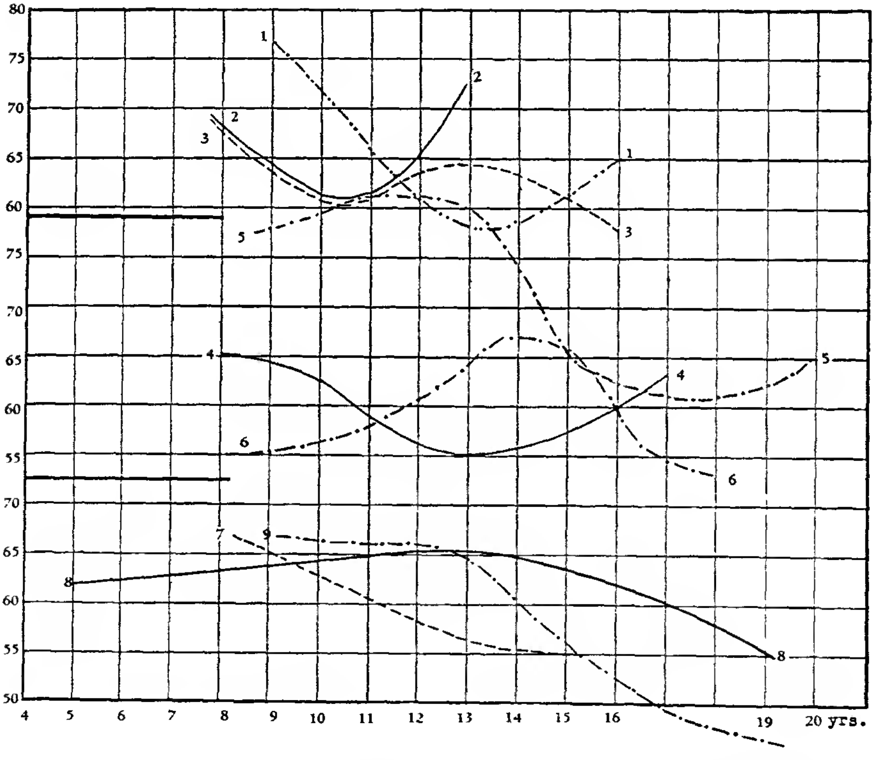

FIG. 64. Change with age of the average percentage ratio of nasal salient to nasal depth, individual males, Nordics. Some figures on Pl. VII. 1, F.V., No. 23; 2, C.S., No. 15; 3, I.J., No. 18; 4, W.M., No. 8 (Fig. 8); 5, F.W., No. 24; 6, J.C., No. 10 (Fig. 14); 7, A.M., No. 7; 8, E.H., No. 41; 9, M.H., No. 2 (Fig. 9).

b''. Individual Data.

Comparative.—Figure 64 shows sundry curves of development of the salience index in boys. The two commonest types are: 1. Decreasing ratio from 9 years, reaching a minimum usually somewhere between 12 and 14 years and then rising slightly, a typical U shaped curve. 2. Increasing to a maximum at about 12 or 13 years and then decreasing to a minimum at about 17 to 19 years.

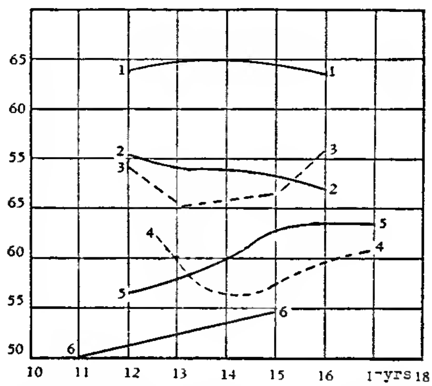

FIG. 65. Change with age of the average percentage ratio of nasal salient to nasal depth, individual females. 1, M.H., No. 51, Nordic, photo, Pl. IV, Figs. 2, 4; 2, C.E., No. 37, Italian; 3, A.F., No. 40, Irish; 4, R.B., No. 14, Nordic; 5, B.A., No. 3, Italian; 6, N.B., No. 11, Nordic.

Figure 65 shows sundry curves of development of the index in girls. Here, in one type, the curve rises nearly uniformly from 11 to 15 or 16 years. In other cases the curve is nearly horizontal or decreases slowly. In two cases out of 6 exhibited the curve is U shaped. The girls usually show no such deep depression in later puberty as the boys do. This is probably due to the fact, brought out in the section on salience, that the nasal salience in girls does not diminish at puberty as it does in boys, since there is no such thickening of the upper lip associated with development of terminal hairs.

5. *Development of Relative Nasal Depth*

(*Percentage Ratio of Nasal Depth to Height*)

This ratio gives the proportions of the nose as viewed in profile.

a. Mass Statistics.

α. *General.*—The mass curve of this index is a complex one. As shown in Fig. 66, during fetal development the

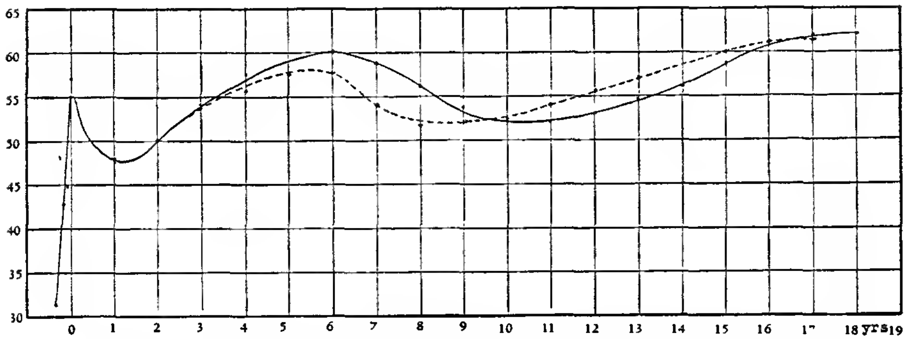

FIG. 66. Average age changes of the percentage ratio nasal depth to height, embryo to maturity, combining data as in Fig. 1. Continuous lines, males; broken line, females. Observations plotted. Ordinates are percentages.

relative nasal depth index increases rapidly from about 33 to 55. This is due to the rapid outgrowth of the nasal protuberance while the nasal height is growing only slowly. Consequently we see at birth and frequently for some months after birth the characteristic infantile nasal papilla (with little development of the face between eyes and mouth) and the low root of the nose (Plate III, 1). Shortly after birth, growth of nasal depth slows up greatly (Fig. 7), while growth of nasal height continues at high speed (Fig. 1). The result is a marked reduction in relative nasal depth which continues into the second postnatal year. This is about the time when the speed of nasal height growth begins to decrease strikingly. From the second to the sixth year the relative nasal depth increases rapidly to a maxi-

mum. After that the index decreases to late juvenility, to rise thereafter with increasing velocity of growth of nasal depth to a maximum in late puberty.

To summarize, fetal life is characterized by relative increase of the depth of the nose; infancy by relative increase in height; childhood by further marked protuberance; juvenility by relative growth in nasal height and finally adolescence and puberty again by relative exaggeration of the nasal horizontal dimension. Thus by a series of changes in profile the adult form of the nose is gradually achieved.

β. Sexual.—Sufficient precise data are lacking as to sex difference in the relative nasal depth index until after 3 years, when a sexual difference becomes plain. The curves of development of the relative nasal depth are much alike in the two sexes; but there is an interesting precocity of the phases in the female as compared with the male. This accords closely with the precocity in sexual development of the girl. The comparison of the sex curves brings out strikingly the fact that the growth-promoting hormones are affecting the velocity of growth of all parts of the body, even the proportions of the nasal profile.

The precocious acceleration in the relative nasal depth in the female results in a decussation of the curves. In this case it takes place first at the 10th year. By 16 years the curve of the female has almost ceased to increase and that of the male decussates with it. Apparently the relative nasal depth is greater in the adult male than female, a return to the juvenile condition.

γ. Social.—Figure 67 shows comparative curves of changes in relative nasal depth for 4 groups. The most normal group (mentally) is represented by the full line. It follows the male curve of Fig. 66 rather closely in most respects except at the younger end (fewer observations). The other two series lie close to the first; one above and one below. The Idiot series has consistently a lower ratio than the more nearly normal LVD$_{II}$ series; *i.e.* their noses are relatively less protuberent.

The Mongoloid series (M) forms quite a different curve. There is no evidence of a rapidly increasing nasal height at 8 to 10 years as shown by the BOA and LVD_{II} series. This accords with the Fig. 7 of Pl. IV. From 12 years

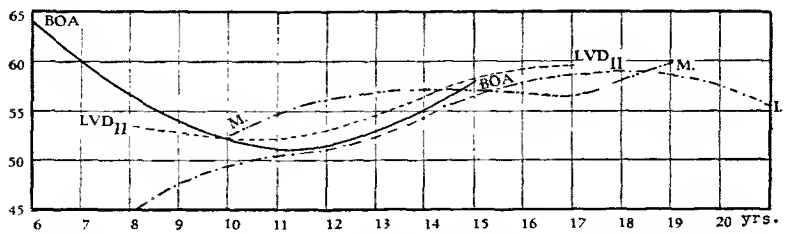

FIG. 67. Change with age of the average percentage ratio of nasal depth to nasal height in four social groups, males, U. S. Nordic. BOA, LVD_{II}, I and M.

onward until 18 years the relative nasal depth index holds steady at about 57, reaching a minimum at 17 years (which may correspond to the normal minimum at 11 years?) and then rises slightly to 60 at 19 years. Thus at 12 years the Mongoloid nose is exceptionally low and protuberant, and at 17 years relatively less protuberant than the normal.

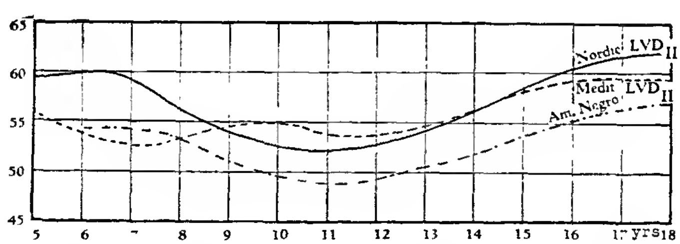

FIG. 68. Change with age of the average percentage ratio of nasal depth to nasal height for three racial groups, LVD_{II} series.

δ. *Racial.*—Figure 68 shows comparative curves of the relative nasal depth index for Nordics, Mediterraneans and American Negroes. The Mediterranean group seems to contrast with the Nordic group somewhat as the Nordic female curve does with the Nordic male, *i.e.* it lies below

the Nordic curve at the extremities; above it in the middle. The Negro group lies for the most part below the other two (smaller nasal depth, *cf.* Pl. IV, Fig. 8), and shows a rather marked increase of nasal height at 11 years.

b. Individual Data.

α. *Comparative.*—The curves of changes of the relative nasal depth with age fall into three main types.

Type I. The U-shaped type, characterized by a high index value (70 to 80) at an early juvenile stage, falling rapidly and nearly uniformly to 13 or 14 years (late adolescent minimum) and then rising somewhat less rapidly to 60 or 70 (Fig. 69).

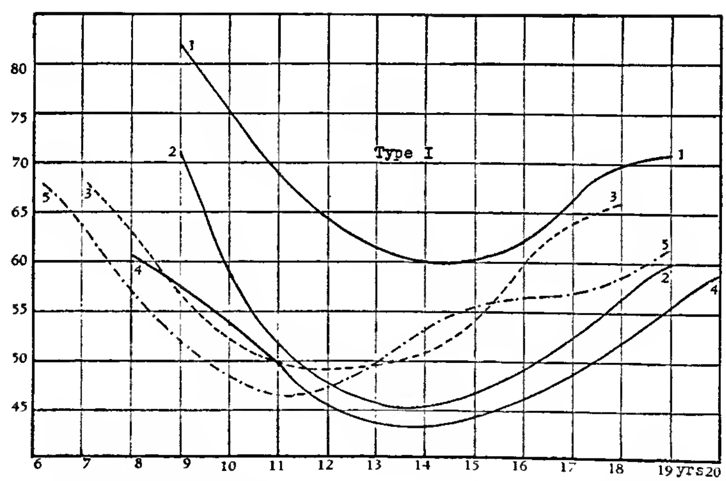

FIG. 69. Change with age of the average percentage ratio of nasal depth to nasal height, individual boys, LVD$_I$. 1, R.H., No. 29, Nordic; 2, H.F., No. 20, Hebrew; 3, M.H., No. 2, Nordic, photo, Pl. VII, Fig. 9; 4, F.W., No. 24, Nordic; 5, J.B., No. 54, Nordic, photo, Pl. VII, Fig. 6.

Type II, the commonest, but the most complex, begins at 8 or 9 years at a medium (or low) index value, reaches a first adolescent maximum at 11–12 years; falls to a pubertal minimum at about 15 years, at 45–50, and then pro-

ceeds rapidly upward to a value of 60–65 before the final adult stabilization (Fig. 70).

Type III, is characterized by slight changes in the index at least until puberty, or about 16 years. Sometimes the curve rises steadily from 8 or 9 years to maturity. The adolescent crest is absent, but in some cases the ratio rises just before maturity (Fig. 70).

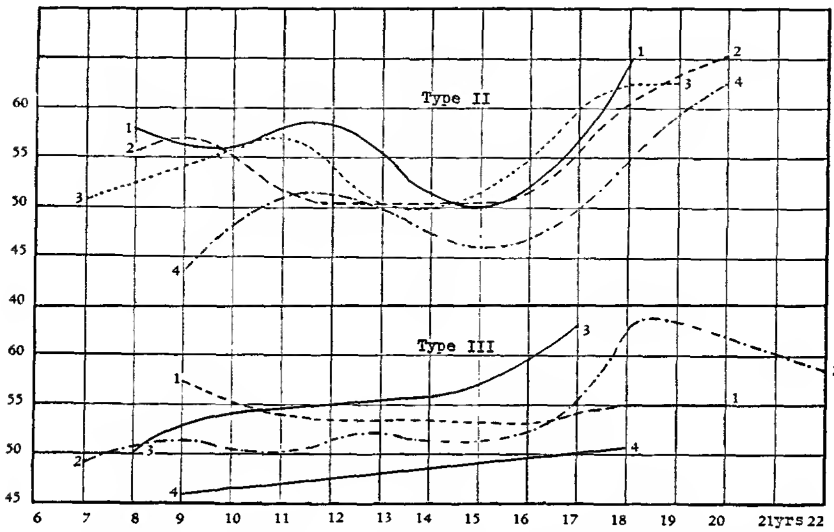

FIG. 70. Change with age of the average percentage ratio of nasal depth to nasal height. Individual, LVD$_I$. Upper curves, type II; lower curves, type III. Above, 1, J.C., No. 10, Nordic; 2, H.M., No. 25, Nordic; 3, A.M., No. 7, Polish; 4, A.DiM., No. 38, Italian. Below, 1, F.C., No. 28, Nordic; 2, C.D., No. 30, Nordic; 3, W.M., No. 8, Nordic, photo, Pl. VII, Fig. 8; 4, J.G., No. 114, Italian.

We note inside each of these types that while the general form is the same the ages at which the maxima or minima are achieved often differ by two or three years. The same pattern of growth impulses is present but its working-out may be anticipated or retarded, as though a stencil were being followed which could be moved to the right or left.

β. Familial.—Figure 71 shows the curve of the nasal depth index for 3 boys of the Hic. family. Here we see illustrated again a common phenomenon. The curves of

the different children have much the same shape and run between nearly the same limits: but the curves are displaced laterally (along abcissæ). The points of inflection of the S-shaped curves are at 11, 13 and 16.5 years respectively.

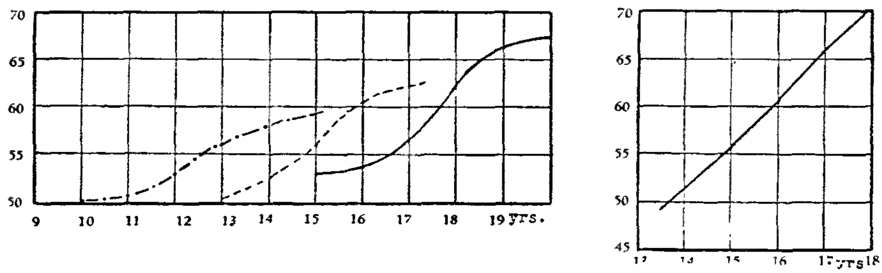

FIG. 71. Change with age of the average percentage ratio of nasal depth to nasal height, family and special cases. Left, three boys of the Hic. family, Nordic; left, Chester; middle, Henry; right, Cecil. Right graph: C.B., cretin, Nordic.

γ. Special cases.—Cretin. Figure 71, right, shows the curve of change in a cretin girl from 13½ to 18 years. She was given thyroid treatment after 13 years of age. The upward trend is striking and abrupt.

6. Development of Nasal Root Depth Index

(*Percentage Ratio of Nasal Root Depth to Apical Depth*)

a. Definition.

This index may be used to trace the relative changes in the ratios of the depths of the nasal apex to nasal root during development. As is well known the root-depth is typically slightly developed at birth (Pl. III, Fig. 1). How do the two dimensions compare in their increase as they progress toward the adult form?

b. Results.

b′. Mass Statistics

α. General.—Figure 72 gives the average curves of changes of this ratio to 17 years of age. The prenatal part

of the curve is based on measurements of only 12 fetuses at the Carnegie Institution's Department of Embryology, Baltimore. The indices are mostly high, about 55. Very

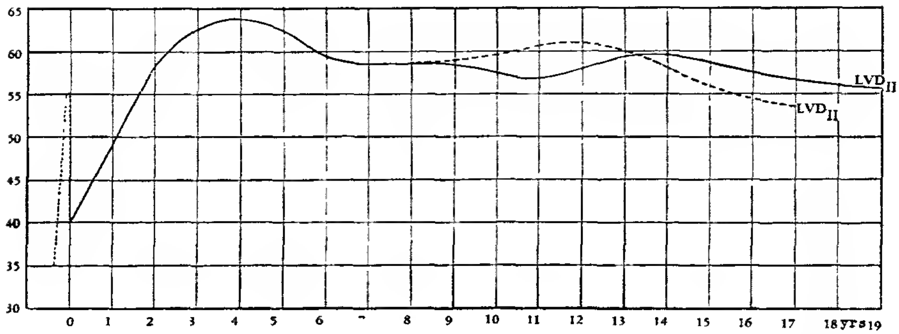

FIG. 72. Change with age of the average percentage ratio of depth of root bridge to nasal depth, embryo to maturity, combining data as in Fig. 1. Nordic male and female. Continuous line, male; broken line, female. Ordinates are percentages.

little stress can be laid on this prenatal part of the curve, awaiting confirmation by a larger series.

At birth the index is about 40. During the first 3 years of postnatal life the nasal root-bridge increases in depth faster than other parts of the nose. After reaching a crest at 4 years (index, 64) growth of the root in depth slacks up while that of the apex grows more vigorously. At adolescence the root depth increases faster but with the onset of puberty the depth of the nose at apex increases relatively vigorously.

β. Sexual.—So far as our data go the index under consideration is not clearly differentiated in the sexes until about 8 years, when the root/apical index increases faster in the female than in the male, apparently anticipating the onset of adolescence with its spurt. After 12 years, when the mean index is 61 the reverse process is initiated; the curve falls to about 53 at maturity. A decussation with the male curve occurs at near 13 years.

In the male curve the adolescent spurt is associated with increasing speed of growth of the root depth. This gives a mean index of about 60 at 14 years. Thereafter the index falls to about 55 near maturity.

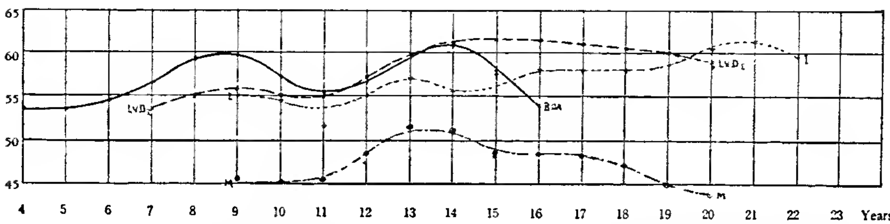

FIG. 73. Change with age of the average percentage ratio of root bridge depth to nasal depth. Four social groups, males, Nordic. BOA, standard; LVDI, high grade feebleminded; I, Idiot grade; M, Mongoloid dwarfs.

γ. *Social.*—The standard BOA curve, of Fig. 73 (which differs from the curve of Fig. 72 because the latter is based in part also on LVDII data), lies above the others at 10 years (due partly to greater root-depth) but falls down more rapidly than the others at 14 to 16 years.

The high grade feebleminded (LVDI) run a similar course except that the curve does not decline as rapidly as that of BOA. The curve of the Idiot series advances more rapidly than that of the LVDI. The Mongoloid index lies, at puberty, for the most part 10 to 15 points below the others. The group is characterized by relatively undeveloped root-depth, one that is under 50 per cent of nasal depth instead of between 55 and 60 as in the other groups (Pl. IV, Fig. 7).

δ. *Racial.*—Figure 74 gives comparative curves of this ratio for the three racial groups. The curve of the U. S. and Nordic group lies highest, owing to the deep root here. The Mediterraneans lie about 5 points below, except before 9 years. The American Negroes lie far below the other groups, due to the low root depth. The curve seems to show that the nasal root is later in developing in the Negro than in the other two groups.

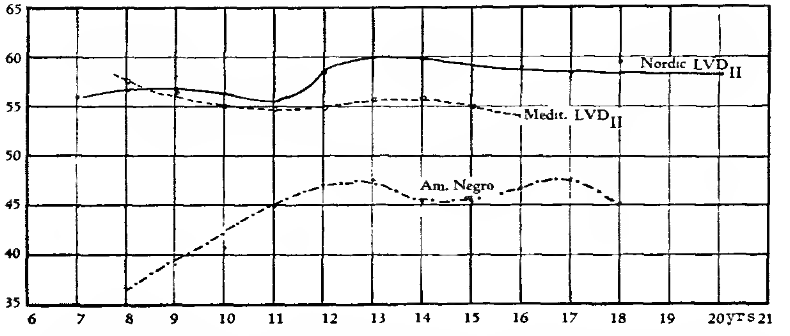

FIG. 74. Change with age of the average percentage ratio of root bridge depth to nasal depth. Two racial groups of LVD_{II} series. Negro, $LVD_I + LVD_{II}$.

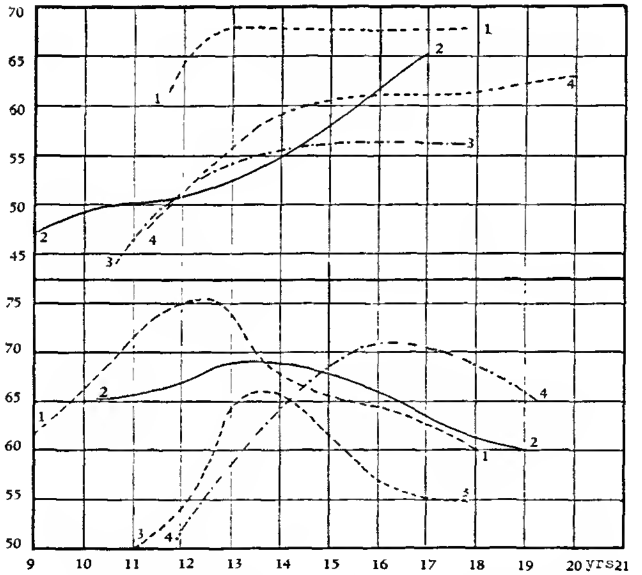

FIG. 75. Change with age of the average percentage ratio of root depth to nasal depth. LVD_I. Individual males. Above, 1, W.B., No. 22, Nordic; 2, W.M., No. 8, Nordic, photo, Pl. VII, Fig. 8; 3, F.B., No. 23, Nordic; 4, M.F., No. 20, Hebrew. Below, 1, J.B., No. 54, Nordic, photo, Pl. VII, Fig. 6; 2, M.H., No. 2, Nordic, photo, Pl. VII, Fig. 9; 3, T.V., No. 65, Italian, photo, Pl. VII, Fig. 11; 4, H.M., No. 43, Nordic, photo, Pl. VII, Fig. 7.

b″. Individual Data.

Comparative.—The changes with age of the root-depth apex-depth index, fall into different types:

Type I. The commonest type of individual change results in the convex-above type of curve. The crest of the curve usually lies between 8 and 18 years and corresponds roughly with the convexity seen in the mass statistics, with its crest at 12 years. Examples of this type are shown in the lower part of Fig. 75. One notes that the index's maxima vary from 66 (curve 3) to 75 (curve 1) and in position from 12½ to 16 years. The maxima correspond in most cases to a period of rapid growth of root-depth.

Type II. In this the curve of change of ratio with age fails to descend even after age 16, in one case even after 19 years. Evidently in these cases growth of apex-depth fails to exceed that of root-depth in late puberty. An example is seen in curve 2, Fig. 75, upper half. Contrast with curve 3, in lower half of Fig. 75 at about the same age. Why the curves of this type fail at 17 years to fall cannot be specifically answered. Doubtless the genes that help mold the form of the nose are different in the two.

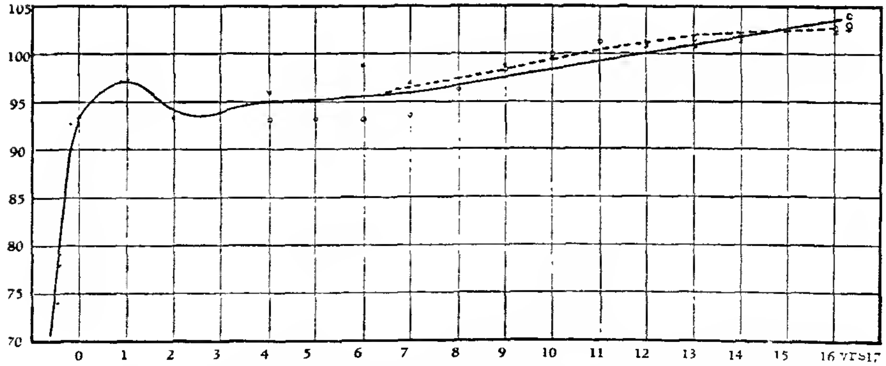

FIG. 76. Change with age of the average percentage ratio of nasal width to interocular width, embryo to maturity, combining data as in Fig. 1. Ordinates are percentages.

7. *Development of the Percentage Ratio of Nasal Width to Interocular Distance*

a. Definition.

Both nasal width at alæ and distance between inner eye angles are dependent on face width, so that between these two dimensions there is a certain correlation. Our problem is, does the relation between the two change during development? Fig. 76 throws light on the problem.

b. Results.

b′. Mass Statistics.

α. General.—In this curve the prenatal part is based on observations of A. H. Schultz (1920, p. 184). As a check and to eliminate the personal equation I have measured a dozen fetuses at the Carnegie Institution's Department of Embryology and found fair agreement with Schultz's figures. These indicate that, beginning with a score of 70 at the 10th week of gestation, the index rises rapidly to 93 or 94 at birth. After birth the rate of increase of the index slows up and, apparently, the index decreases from 12 to 24 months. This is a curious notch in the curve which may be due merely to insufficient numbers but seems to be a significant feature of the curve. From 3 years (curve at 94) the curve rises, on an average about 1 point p.a., reaching a mean position between 100 and 105 at maturity. In other words, in the young fetus the nasal alar width is small as compared with the distance between the inner angles of the widely separated eyes. As the eyes move into a more strictly frontal position the nasal width in relation to them increases greatly, and at one year (postnatal) the two dimensions are nearly equal. Thereafter as the eyes continue to approach the ratio runs up to 103 or higher.

β. Sexual.—The curves for the sexes run close together until at about 7 years the ratio in the adolescent female comes to exceed by a point or two that in the male. Since the nasal width is smaller during this period the greater

index must depend on a narrower distance between the eyes and such a smaller interocular distance at 7 to 14 years is characteristic of the female.

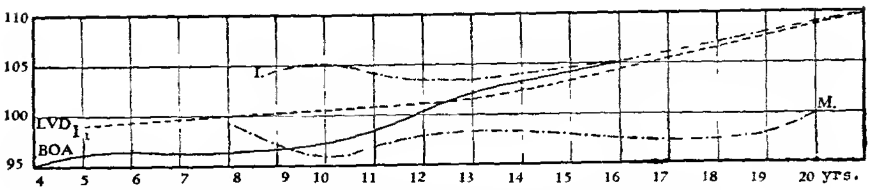

Fig. 77. Change with age of the average percentage ratio of nasal width to interocular width. Four social groups, Nordic, males.

γ. *Social.*—Figure 77 gives the change of index with age for 4 groups, BOA (standard), LVD$_I$, I and Mongoloid. From the curves it appears that at age 10 the ratio is largest (105) for the I series, next for the LVD, still lower (97) for the BOA series. The M series lies at the bottom at 95.5. This is also the order of nasal width. Apparently the ratio increases in all cases with general development except that the Mongoloid dwarfs, with their almost fetal-like noses, form a group by themselves, with a markedly low index. By maturity the indices of all groups, except the Mongoloids, are equal. It may be added that in the Mongoloids the index is, at 13–18 years, 2 to 3 points larger in the female than in the male.

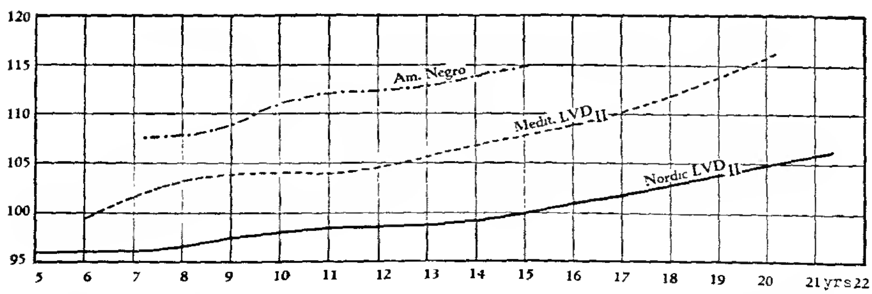

Fig. 78. Change with age of the average percentage ratio of nasal width to interocular width for two racial groups of LVD$_{II}$ series and all Negroes.

δ. *Racial.*—Figure 78 shows the position of this index at different ages for the three racial groups: U. S. and Nordic, Mediterranean and Negro. The Nordics have the smallest ratio at 15 years, about 100. The Mediterraneans at the same age stand at 108; the American Negro at 115. The high position of the Negro is, of course, due to the high nasal width of this race, sufficient to counterbalance the great interocular distance.

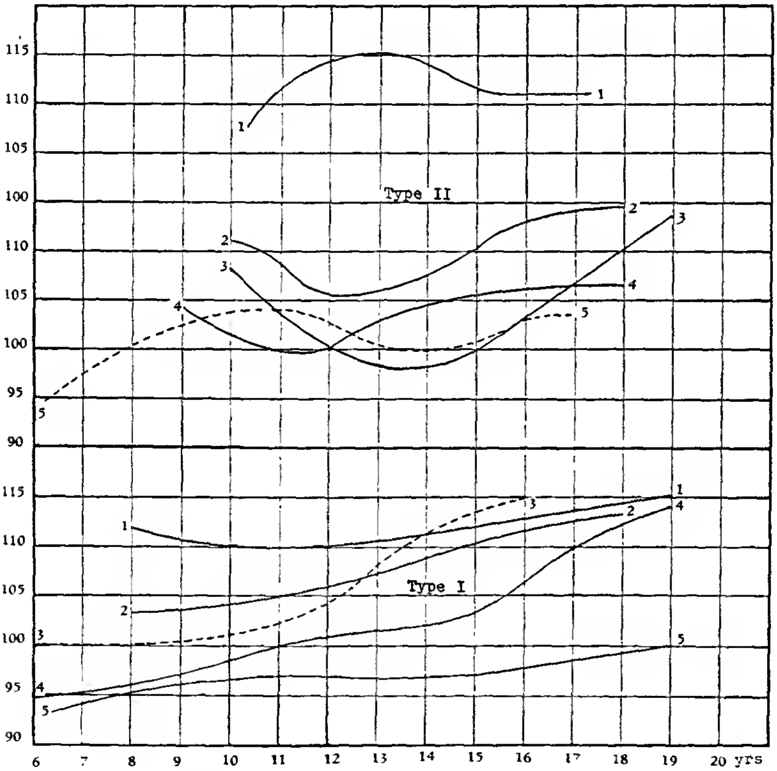

FIG. 79. Change with age of the average percentage ratio of nasal width to interocular width for male individuals, illustrating different types. 1, V.R., microcephalic. Above: *Type II*, 2, M.G., No. 95, Italian; 3, H.M., No. 43, Nordic, photo, Pl. VII, Fig. 7; 4, T.V., No. 65, Italian, photo, Pl. VII, Fig. 11; 5, J.G., No. 114, Italian. Below: *Type I*, 1, M.H., No. 2, Nordic, photo, Pl. VII, Fig. 9; 2, J.B., No. 54, Nordic, photo, Pl. VII, Fig. 6; 3, E.C., No. 58, Nordic; 4, M.F., No. 20, Hebrew; 5, W.M., No. 8, Nordic, photo, Pl. VII, Fig. 8.

b″. Individual Data.

Comparative.—Two principal types of change of the index are seen in children of European origin.

Type I. Mostly increasing with age, though rarely in straight line fashion (Fig. 79). The position of the curves is very variable, from curve 5, which at 10 years has an index of 96.5 to curve 1 which at the same age has an index of 110. This type is the commonest one among Nordics.

Type II. This is the U-shaped type, very strikingly shown in curve 3. From 108 at 10 years the curve descends to 98 at 14 years and then rises to 114 at 19 years. The depression is rarely so marked as this. This type in early years (6 to 10) may show a rising trend as we see in the case of curve 5. This condition is faintly shown in the mass curve (Fig. 76) at those ages.

8. Development of the Percentage Ratio of Root Depth to Interocular Distance

a. Definition.

This ratio includes two dimensions that change rapidly during early development. The root depth is often 5 mm or less at birth and may increase to 21 mm or more by maturity. The inner eye angles are far apart when first detectable in the embryo; but even though the eyes approach relatively the absolute distance increases with age.

b. Results.

b′. Mass Statistics.

a. Sexual.—Figure 80 gives a mass curve showing changes of the index with age. Since embryologically the root of the nose is elevated rather late, while the eyes are very far apart, the initial index, at 8 to 10 weeks, is nearly zero. By 20 weeks it is about 20 and at birth about 30. The increase of the ratio now slows down, until, in the male at 6 to 10 years, it remains for some time practically constant. At 12 to 14 years the ratio rises again at about 2

points p.a. as the nasal root rises; and after adolescence the ratio increases slowly about half a point p.a.

In the female the curve of index-change with age runs in general a parallel course with that in the male. During

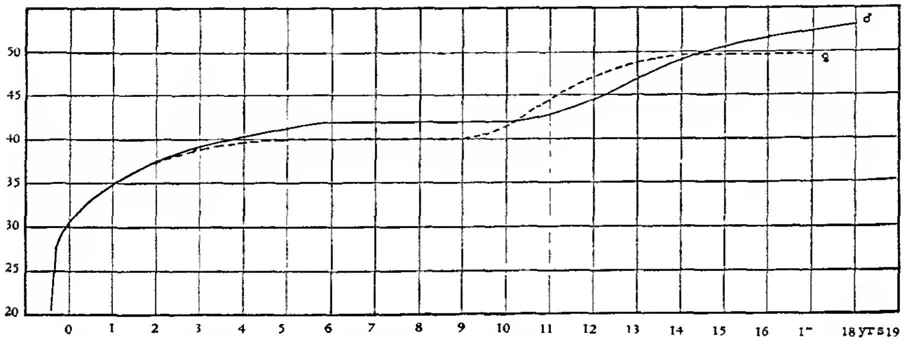

FIG. 80. Average change with age of the percentage ratio of root depth to interocular width. Source of data as in Fig. 1. Ordinates are percentages.

the steady period, 5 to 9 years of age, the ratio seems to be about 2 points less than in the male, partly due to the fact that the root-depth is at this age about one-fourth less in the female than the male. At about 10 years the adolescent spurt begins and a decussation of the sex curves occurs at about 10 years. This spurt then dies away while the male curve is progressing rapidly so that a second decussation occurs at about 14 years. After this the mean age curve of the female becomes level again.

β. Social. The LVD$_{\text{II}}$ series, being the latest measured and consequently the most reliable as well as the most numerous, gives a nearly straight line of the increasing ratios with age (Fig. 81). The standard (BOA) boys, on the other hand, show a marked increase of the ratios from 11 to 14 years, amounting to 11 points or 3.67 points p.a. In the I series, as the next lower social grade, the index falls definitely below the others, until 15 years, at any rate. The Mongoloid curve lies for the most part below the others except for the mean spurt at 13 years, which is hard to understand. Certainly, in general, the Mongoloid dwarf

children are characterized like typical Chinese by shallow root and broad eye interspace.

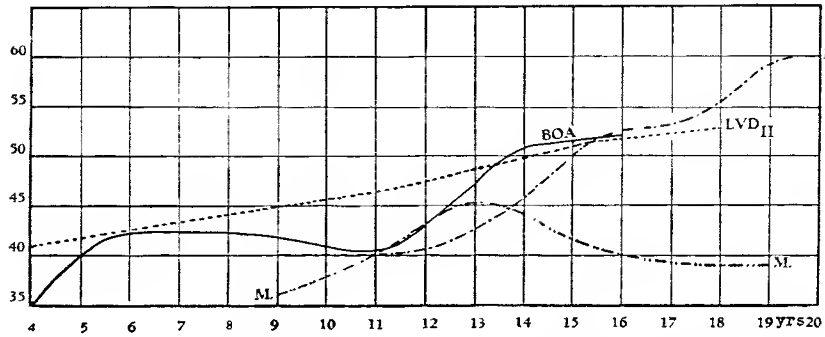

FIG. 81. Change with age of the average percentage ratio of root bridge depth to interocular width for four social groups, Nordic, males.

γ. *Racial.*—Of the three groups considered in this study the Nordics have the highest index at all ages (Fig. 82).

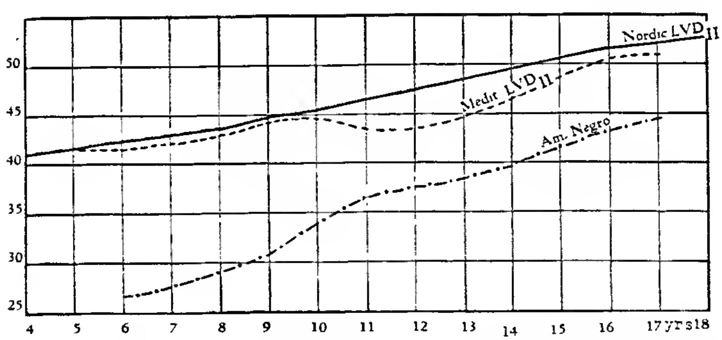

FIG. 82. Average change with age of the percentage ratio root depth to interocular width, for three racial groups.

The Mediterraneans are very similar except that the ratio seems in our sample to slump at 11 to 12 years. In the Negro the ratio rises rapidly 2.5 points p.a. from 8 to 11 years, as in the Mongoloids. But beyond 11 years it continues to rise at the rate of about 0.5 points p.a. It still keeps strikingly below the white ratio. This low ratio is largely due to the great interocular distance.

b″. Individual.

Comparative.—Figure 83 gives curves showing changes in the root depth/interocular index at various ages for 5 LVDI individuals of Nordic stock. The profiles of the nose of 4 of these individuals are shown in Pl. VII, Figs. 6, 7, 8, 9. The difference in height at nasal root of curve 3, Fig. 9 (No. 2), and curve 2, Fig. 7 (No. 43), is striking in the photographs at 19 years. At 12 years the difference between these two boys was less striking (Pl. VIII, 7, 9).

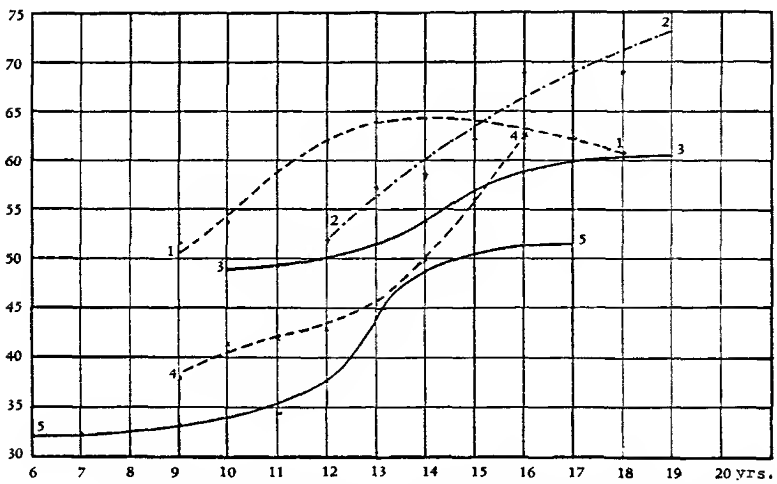

FIG. 83. Change with age of the average percentage ratio of root bridge depth to interocular width, individuals, males. 1, J.B., No. 54, Nordic, photo, Pl. VII, Fig. 6; 2, H.M., No. 43, Nordic, photo, Pl. VII, Fig. 7; 3, N.H., No. 2, Nordic, photo, Pl. VII, Fig. 9; 4, W.M., No. 8, Nordic, photo, Pl. VII, Fig. 8; 5, R.O., No. 110, Nordic.

Comparing these curves we see that they do not run parallel. Thus curve 2 cuts across curve 1 and (prolonged) across curve 3. Indeed, 3 main types are distinguishable:

Type I. The concave-above type, seen in No. 4, Fig. 83, also curve 1, M.G. (No. 95) of Fig. 84.

Type II. The convex-above type, seen in curves 1 and 2 of Fig. 83, and curve 3, T.V. (No. 65) of Fig. 84. Also, strikingly, in curve 2, H.T. (No. 3) of Fig. 84, a colored boy.

Type III. The S-shaped type is seen in curve 5, R.O. (No. 110) of Fig. 83, and curve 3, M.H. (No. 2) of same figure. These show their inflection at ages 13 and $14\frac{1}{2}$ respectively. These are about the ages of their adolescent

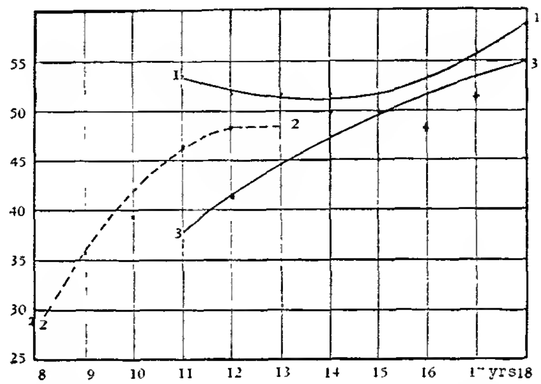

FIG. 84. Change with age of the average percentage ratio of root bridge depth to interocular width, individuals, males. 1, M.G., No. 95, Italian, acromegal; photo, Pl. VII, Fig. 10; 2, H.T., No. 2, colored; 3, T.V., No. 65, Italian, photo, Pl. VII, Fig. 11.

spurts of growth, which occur in R.O. at 12 years and in M.H. at 15 years.

In general an upward turn in the root depth/interocular distance ratio follows or accompanies a major spurt in stature growth.

D. COMPARISON AND DISCUSSION

1. The Mass Curves of Growth

a. Absolute.—The mass curves are in general interesting because of their smoothness. They look more important than the individual curves. This smoothness is largely a statistical rather than a biological phenomenon. However, they do tell much of biological interest and may be used when checked by the individual curves.

The curve of nasal height (Fig. 1) is, roughly, a segment of a sigmoid curve. Actually it does not start at zero nor show the slow increment at low values of a typical sig-

moid. Also, one sees a wave-like change in slope between 4 and 17 years, which is partly explained by the abrupt changes in the individual curves at ages varying from 12 to 15 years that are doubtless related to the adolescent spurt of growth. The prenatal passes without break into the post-natal part of the curve.

The curve of nasal depth (Fig. 7) also advances roughly as a sector of a sigmoid curve. However, although it starts at zero, growth at the very beginning is extremely rapid, and obviously has not to do merely with simple cell-multiplication processes. This curve passes from prenatal to post-natal stages without a break, but within a month or two shows a slight slowing up of growth during the remainder of the first year and later. There is an acceleration at adolescence.

The mass curve of growth of nasal width (Fig. 14) is also one which does not begin at zero. It increases in prenatal life very rapidly, passes without a break into post-natal life and after 2 years grows at a uniform slow rate, with a slight acceleration at adolescence.

The mass curve of the nasal salient (Fig. 21) shows, better than the curve of depth, a curious behavior during the first post-natal year. The salient almost ceases to grow doubtless owing to the great development of the maxilla and its dentition. But this is much less in the human child than in the ape with its extraordinary development of the maxilla and its included teeth. There is a striking acceleration in rate of growth at 7 to 9 years.

The mass curve of the growth of depth of the nasal root (Fig. 28) is no less striking. Growth slows down at birth for the first year; is accelerated during the second year; slows down during the third year; grows uniformly and slowly until adolescence when it increases rapidly during the 13th year until about the time of the adolescent spurt.

A contemplation of these mass curves of absolute dimensions shows that while with growth of the individual every one of them increases from birth to maturity, and

every one grows with the greatest rapidity (like the body as a whole) during intrauterine life; the slope of the mass curve not only quickly diminishes within a year or two after birth but it shows important changes in slope associated with individual physiological adaptations. The mass curve indicates no single reaction starting at one point and developing to a maximum velocity and then fading away. The result is far more as if first one group of cells (and tissue) and subsequently other groups became involved. The growth of any dimension is like that of the organism as a whole, it does not merely grow larger but it is undergoing differentiation at nearly every stage.

b. Indices.—The mass index curves, which record the changes in ratios of selected pairs of dimensions have varied forms. The nasal index (percentage nasal width to nasal height) has at the start a very high value (Fig. 33), as it has in the anthropoids. The ratio falls rapidly and at birth is slightly below 100, already less than that of any anthropoid. Gradually, the slope of the ratio change slacks up, becoming less than ½ point per annum. However, it decreases rapidly again at the beginning of adolescence, possibly increasing slightly as puberty sets in.

The mass curve of age-changes in the ratio of nasal (apical) depth to nasal-width is shown in Fig. 42. The curve is a rough segment of a sigmoid curve which begins at about 38 per cent, changes most rapidly at the start, shows no break at birth, slows down to a constant ratio of 71 at 4 to 6 years, increases thereafter and shows a very great increase at 15–17 years, at a time when the mean apex depth is increasing and the mean nasal width not.

The mean change with age of the ratio nasal width to bizygomatic width starts at about 34.5 per cent, falls with great velocity to 25 per cent at birth (Fig. 54). This is because the nares are drawing relatively closer together during this period. There is no break at birth, but the ratio continues to diminish (though more slowly), to the third year. Then there is a rise to the 6th year, a fall and

then a rise again to maturity. This irregularity is apparently chiefly due to varying rates of growth of the width of the face at the level of the zygomas.

The average change with age of the ratio of nasal salient to depth (Fig. 61) gives a remarkable curve. Before birth the nascent nasal salient increases as compared with depth so that the ratio doubles in 7 months. But immediately at birth a great change occurs in the direction of the curve owing to the cessation of growth of nasal salient for a year or two (Fig. 21) probably due to the thickening of the maxillæ as they develop. In the male curve the ratio increases again temporarily at 10–11 years after the upper permanent incisors have been cut.

The change with age of the ratio of nasal depth to height is a striking one (Fig. 66). The ratio increases rapidly during intrauterine life up to 55 per cent; but at birth it begins to diminish almost as rapidly. By the end of the first year the ratio has fallen to 48 per cent. The ratio thereupon rises during childhood and reaches a maximum at 6 years. Thereafter the ratio diminishes to the beginning of adolescence and rises during adolescence and puberty. By referring to Figs. 7 and 1 it is seen that the mean depth has a maximum at 6 years and mean height a low point at 5 or 6 years. It is the ratio between them that accounts for the high point at 6 years in the male. One sees how intricate is the change of slope of the index even in masses. Naturally, the individual curves are far more intricate.

The changes with age of the mean nasal ratio of root-depth to apex-depth are shown in Fig. 72. The data on prenatals are few, so this part of the curve is tentative, but apparently the root depth rises to over 50 per cent of apical depth by birth; diminishes to 40 per cent at birth, thence increases greatly to 63 per cent by 4 years; and then slowly diminishes to maturity except for a slight increase at the mean time of the adolescent spurt, when root-depth increases faster than apical depth. The rapid growth of root

as compared with apex up to 4 years transforms the baby's nasal papilla to a real human nose,—a change accentuated at adolescence.

The mean age-changes of the index, ratio of nasal width to interocular width, are likewise complicated (Fig. 76). Apparently during prenatal life the inner angles of the eyes approach each other relatively faster than the nose narrows at the wings. During the second year with the great increase of the face the relative change is reversed; but from the third year on the nasal width increases faster than the interocular width.

The mean age-changes of the ratio, root depth to interocular distance, are shown in Fig. 80. This ratio increases continuously from the first appearance of the outer nose to 18 years, except for the period 6 to 10 years when there is little change. That is, assuming the interocular distance to remain constant, the root depth regularly increases. Or, better, the rate of increase of the root depth is greater than, or at least equal to, that of the distance between the inner angles of the eyes. The root-depth makes an especially fast relative growth at adolescence. Always the root-depth seems to be growing at the expense of the horizontal elements of the face between the orbits.

The mass index curves deal not with growth but with differentiation or with relative growth of dimensions. Consequently we have no reason to look here for logistic curves, however modified. Of 8 mass index curves 6 ascend with age during prenatal development. Of the others, the percentage ratio of nasal wing width to bizygomatic width (Fig. 54) descends abruptly. This is because prenatally, and until 3 years postnatally, the face is widening much more rapidly than the nose, or in other words, the nose is becoming narrow relative to face. The embryological process by which the nasal pits from a lateral position on the head come to lie close together in front is not completed until 2, 3 or more years after birth. Phylogenetically, also, the narrow nose is a recent feature.

Percentage of width to height (nasal index, Fig. 33) descends less steeply. Its trend shows that the nose grows in height faster than in width from an early embryonic stage to adolescence. This is partly because the first rudiment of the nose is a papilla wider than high. The high nasal "bridge" or ridge is apparently a recently evolved human trait.

In the other 6 cases: depth by width (Fig. 42), salient by depth (Fig. 61), depth by height (Fig. 66), root depth by apical depth (Fig. 72), width by interocular breadth (Fig. 76), root depth by interocular breadth (Fig. 80), the ratios increase during intrauterine life; *i.e.* the smaller dimensions such as salient, root-depth, increase relatively more rapidly than the larger dimensions, such as nasal height and interocular width.

2. The Neonatal Period, a Critical One in Growth of the Nose

The neonatal period is a critical one in the development of certain dimensions of the outer nose. Before birth the nostrils are not functioning. Nevertheless, rapid changes in outer nose are occurring. From birth the nostrils are exceedingly important as the respiratory doorway and the outer nose is, as it were, the frame of the door.

The growth of the following dimensions continues rapidly for some weeks after birth: nasal height (Fig. 1), apical depth (Fig. 7) and nasal width (Fig. 14). On the other hand, the nasal salient almost stops increasing during the period from birth to 19 months, due to maxillary thickening (Fig. 21).

Of the 8 ratios, three seem to involve no abrupt change at birth, namely, nasal index (Fig. 33), nasal depth by width (Fig. 42) and nasal root-depth by interocular distance (Fig. 80). In the case of the ratio, nasal width by bizygomatic width (Fig. 54) the reduction in slope has already begun to appear before birth.

If we arrange the 8 ratios in accordance with the degree of change of slope at or about birth, placing the least change first, the series will run like this:

Slope	
—	width by height (Fig. 33)
+	depth by width (Fig. 42)
+	root d. by interocular d. (Fig. 80)
—	width by bizygomatic w. (Fig. 54)
+ —	width by interoc. (Fig. 76).
+ —	salient by depth (Fig. 61)
+ —	depth by height (Fig. 66)
+ —	root depth by apic. depth (Fig. 72)

The ratios of width to height and of width to bizygomatic distance are regularly decreasing ones. The decrease in both cases is due to the relative approach of the nasal wings during this period. This is one of the most obvious of embryological changes, inasmuch as the nasal pits arise laterally and eventually come, represented by the nostrils, close together. This movement follows the line of phylogeny also. In the case of the ratio of nasal width to bizygomatic width a reversal of slope occurs at the third year.

The ratios of depth to width and root depth to interocular distance are regularly increasing. This increase follows from the fact that the nose depth as viewed in profile starts ontogenetically from zero and eventually becomes perhaps the most prominent of human facial features. Ontogeny again recapitulates phylogeny, since the nose of the anthropoids and lower Primates has little depth.

Of the four ratios that change from a rising to a falling gradient within a few months after birth, width to interocular distance (Fig. 76) shows only a relatively slight temporary change of slope. Apparently this is due to the fact that in the second post-natal year the interocular region (Fig. 28) after a temporary check, increases rapidly and at the same time growth in width of the whole nose slows up (Fig. 14). In the third year width tends to fill out again so that the slope of the ratio is reversed—increasing; also the relative interocular distance apparently

tends to come to a stand-still as this part of the face has become fairly well established.

The second ratio the slope of which shows change of sign shortly after birth is salience to depth (Fig. 61). Until shortly after birth salience has been increasing faster than depth. Then the reverse process sets in, probably consequent upon thickening of the maxilla as teeth develop. With slight remission (in the juvenile male) the nasal salience becomes all the way to maturity a diminishing fraction of nasal depth.

The third ratio shows a quick change of value with age; it is depth to height (Fig. 66). Up to birth, depth (starting at zero) increases rapidly (Fig. 7); at birth slows down strikingly, while height (Fig. 1) lessens very little. The curve suggests that before birth the developmental potentialities are being especially directed toward getting the external nares developed to a size at which they can function adequately in respiration. After breathing has begun growth of the nostrils slows up while the other dimensions of the face are relatively accelerated. This change of slope seems to have a physiological rather than a phylogenetic meaning.

The fourth ratio with marked change of slope at birth is root depth to apical depth (Fig. 72). This measures the relative speed with which the upper and the lower parts of the nose rise from the face plateau. Since the ratio of prenatal portions of the line is based on very limited data (16 fetuses) it is drawn dotted. Certainly from 20 weeks up to the 10th lunar month (but not including the latter) apical depth barely doubled[1] while root depth increased at a faster rate, viz., 0.9 mm per month (Fig. 28). After birth the rate of root depth increase, during the first post-natal year, falls to 0.25 mm per month (probably due to rearrangement of bones about the orbit), while apical depth is

[1] It is possible that in the fetuses studied, which had been kept in formalin in a tank, the apex had become by mechanical pressure more depressed than the root. In the living child at birth the truer relations were suddenly realized.

increasing at an even slower rate. It is difficult to account for the form of the ratio curve at its beginning on biological grounds.

To recapitulate, in general, the curves of the ratios are not curves of growth but express graphically changes in proportions with age. They enforce this conclusion: that the various nasal dimensions and those of the adjacent bones of the face are undergoing a constant rearrangement and adjustment. The growth of the nose is accompanied by marked *differentiation* of parts of the nose and face. There is a real metamorphosis of the face at this time. There is a quick adaptation to the new conditions imposed by aerial life. And the outer nose as the entrance to the respiratory system has to make a marked readjustment in form to meet the new burden thrown upon it.

3. The Outer Nose in Relation to Sex

The outer nose as the doorway to the respiratory system performs the same function in the two sexes and in so far should have the same form. This study shows that the growth curves of the sexes are not far apart and, for the most part, show the same angles of slope. However, the dimensions of the female nose are, from birth on, less than those of the male. This is, however, related closely to the difference in body size of the sexes. Relatively to stature the height of the nose is about the same in the two sexes. And the precocious adolescence in the female leads the dimensions in the female to approach those in the male at that period (Figs. 14, 33).

The outer nose is more than the respiratory gateway. It is an important facial ornament and plays an important rôle in expression of the emotions in men as in apes (Pl. II). As the proportion of persons with "adherent ear lobes" was found by Pöch ('26, p. 320) to be greater among the females of Wolhynia than the males (40:25) so there might be, and perhaps is, a difference in proportions of the nose.

A study of the 8 curves of nasal proportions shows that, on the average, sexual differences in certain of them at or near maturity are often more striking than the size differences. Thus, in the male the ratio is the greater in depth to width (10 points, Fig. 42); root-depth to apical depth (5 points, Fig. 72); root depth to interocular width (4 points, Fig. 80). The other differences are probably not significant.

On the other hand, the nasal index of the female averages 4 points greater than of the male (Fig. 33); and this has been found to hold true of the Badeners with a ratio of 66:76 (Fischer teste Martin, '28, p. 551), but not of all Europeans. Since this difference in the nasal index is found at all post-natal ages (Fig. 33) it is probably a real sex difference. Compared with the male the female has a nose that is lower, of about the same width. Hence the female has proportionately to nasal height and to stature the wider nose.

In the ratio of salience to depth the mean female in our data exceeds the male, slightly at 18 years, markedly from 5 to 8 years. The inferior position of the male at the time when the permanent incisors are cutting may be due to the larger teeth of the male; and the lower position of the male ratio after 14 years may be due to the thickening of the upper lip (and partial burying of the nasal salient) with the development of the thick terminals (bristles) there (Fig. 61).

Other striking deviations of the sex curves are found in Fig. 66 (ratio of depth to height) where the female ratio is at first below the male's, but at 10 years rises above it to pass below (probably) at 17 years. This is probably related to the smaller nose height in the female (Fig. 1) at 13 years at a time where there is no great difference in depth (Fig. 7). Horizontal dimensions of the nose, as of other organs, show less sexual difference than vertical ones.

The precocity of the average female ratio curves is not less striking, in Fig. 72 (root-depth to apical depth) and

Fig. 80 (root-depth to interocular width). In general, the female nose and face undergo differentiation earlier than the male. The conclusion to be drawn from those differences in the course of development of the outer nose is that the growth of the skeletal and cartilaginous elements involved are responsive to the growth hormones that influence the body in general and especially the long bones of the appendages.

4. The Outer Nose in Relation to Social Type

This rubric has been utilized to cover the studies made on persons from various social strata, whose most striking differential is intelligence, to whatever causes that differential may be due. The classes recognized are (1) BOA of normal intelligence: (2) LVD$_{II}$ and (3) LVD$_{I}$ selected children in an institution for mental defectives; among the best developed of such children, called morons: (4) I, idiot or imbecile children, cooperative and without neurological defect and (5) M, mongoloid dwarfs. Imbeciles are often pictured as having exceptionally large noses. Mongoloid dwarfs have slightly developed noses.

In nasal height (Fig. 2) the first 4 groups differ little, though the defectives, despite lower mean stature, have slightly higher noses than the standard. The nose of the mongoloids, reaching a height of only 45 mm at maturity, does not get beyond the stage of a 12-year old boy.

In nasal depth (Fig. 8), groups 3 and 4 stand higher than the standard; group 5 is again below, by 2 to 3.5 mm.

In nasal width (Fig. 15) the groups 3 and 4 at an early age slightly exceed the standard but later fall below. The M group is constantly below standard.

In nasal salience (Fig. 22) after 12 years the groups 3 and 4 lie below the standard; group 5 lies far below.

In root-depth (Fig. 29) at 12 years the mean of the standard is greatest, and the mean root-depth decreases with decreasing intelligence. In the mongoloids the root-

depth remains fairly constant at 10 to 12 mm being that of the mean child of 2 to 3 years. In extreme cases a root depth of only 7 mm has been measured on juvenile or adolescent mongoloids—a neo-natal depth.

While the mean nasal index (Fig. 34) of group 3 is close to the standard, that of 4 is about 2 points higher and that of 5 (M) is 2 to 6 points higher. This is, of course, because the nasal height is so small. The childish high mean index of 74 per cent is retained by the M group with small fluctuations to maturity.

The mean depth/width index (Fig. 43) is lowest in the standard series because of its small depth and medium breadth. There is a decussation of the curves at around 12 and 13 years; but, after onset of adolescence, excepting the M series, the I series has the largest index and series 3 the next largest. The curve of position of the M series intertwines the others.

The nasal width/bizygomatic index (relative nasal width, Fig. 55) is smallest before 14 years for the standard series, due chiefly to its large mean bizygomatic width. This condition is an "advanced" one, since large relative nasal width is a fetal condition. The I series has the largest mean index before adolescence, the smallest at maturity. The M series is at most ages intermediate.

In the salient/depth index (Fig. 62) the BOA series occupies the highest position, reaching 69 per cent, owing to the large salience of the standard nose and its small depth. This might be regarded as an infantile condition. At 14 years the series 2 and 3 occupy a median position with series 4 at the bottom; the M series is usually near the bottom of the scale with a mean index that is usually below 60.

In the depth/height index at (Fig. 67) 14 years the M series occupies top place, which might be accounted for on the ground of retarded adolescent spurt. At 17 years it lies at the bottom—the age of the adolescent spurt when the nasal height is typically large. The other series have

passed out of this spurt period and have a high position due to the increasing mean apical depth as maturity is approached.

In the curve of the index root-depth apical depth (Fig. 73) the standard series occupies a high position until 13 years, chiefly owing to the great root-depth in this series (Fig. 29). The M series lies at the bottom, after 12 years of age, owing largely to its very shallow root. The other series are intermediate.

In the width/interocular ratio (Fig. 77), the M series lies at the bottom, at around 97 per cent, owing chiefly to its narrow nose and also to a relatively large interocular width. The I series has a ratio around 105. The other series are intermediate.

In the root depth/interocular ratio (Fig. 81) the M series lies at or near the bottom, at around 40 per cent, and the series 2 and 4 near the top (40 to 60 per cent). In the BOA series a great advance from 40 to 51 per cent is made in this ratio at adolescence since the interocular width is then relatively small, while the root-depth is large.

To sum up. The mongoloid nose is low, shallow, narrow, unprominent, and has a shallow root. The standard nose is opposite in most respects. The nose of the feeble-minded, on the average, is nearer the standard nose than the mongoloid. It is fairly high, deep at apex and root. In all these dimensions the M series is characterized by slow growth from 6 years to maturity, and in it the adolescent spurt of growth is much delayed.

Considering indices, the mongoloids stand apart in their large nasal index, narrow-low noses, small root-depth by apical depth, small ratio of both breadth and root depth to interocular distance. The standard series stands apart in small nasal index, and depth width, width bizygomatic width, apex depth salient ratios. The series 2, 3 and 4 for the most part occupy intermediate positions, but the large nasal index of the imbeciles is worthy of note.

5. The Outer Nose in Relation to Race

For the purposes of this study three "racial" groups are recognized: Nordic (into which some of U. S. origin but of mixed ancestral stock are placed), Non-Nordic (combining chiefly Italians and Jews, also called Mediterranean) and American Negro (carrying more or less White admixture). The "Mediterranean" group is open to criticism; curves have been made for Italians and Jews separately, but since the numbers of the separated groups are small and the curves run fairly close together, it was decided to unite them in one group.

a. Means of absolute measures.—Nasal height (Fig. 3). The Nordic series shows, at all ages studied, the lowest nose and the Mediterraneans the highest, while the Negro occupies an intermediate position. While in the Nordic series the advance with age is practically at about 1.5 mm p.a., in the Mediterranean series the advance is about 2 mm p.a. between 8 and 12 years. The Negro rate is intermediate.

In nasal depth (Fig. 9) the Nordics are generally on top (except between 9 and 13 years when the Mediterraneans exceed them). The Negro is intermediate.

In nasal breadth (Fig. 16) the Negro, of course, forms a quite isolated series. The Mediterranean series has the narrowest nose. These relative positions are held at all ages studied; we know from Schultz's ('20, p. 180) studies that they hold even prenatally.

The nasal salient (Fig. 23) also is greater in the Negro. The two European series do not differ significantly in this respect.

The root depth (Fig. 30) is lowest in the Negro (an ontogenetically early condition) and highest in the Nordics.

b. Mass curves of the ratios.—The curve of nasal index (Fig. 35) of the Negro again is quite widely separated from that of Europeans; at 12 years being 80.7 as opposed to 67.6. This is, of course associated with the great nasal

width of the Negro's nose. Between Nordic and Mediterranean there is an interchange of position at 12 years, the Nordics passing the Mediterraneans.

The nasal depth/width ratio (Fig. 44) is, corresponding to its great width, very low in the Negro; in the female being at 12 years 64, as compared with Europeans, 81. The male Nordic index is constantly slightly above that of the Mediterranean at 12 years.

The nasal width/bizygomatic width index (Fig. 56) in Negroes is, on account of the great nasal width in that series, quite in a class by itself, being 3 or 4 points above that of Europeans. As in the nasal index and female depth/width ratio, the Nordic curve decussates with the Mediterranean during adolescence.

In the curve of salient/depth ratio (Fig. 63) the Negro series stands a little below the others during adolescence, but it is doubtful if the difference is significant since the numbers are small.

In the curves of nasal depth to height ratio (Fig. 68) the Negro's is by about 2 to 5 points the lowest from 8 years onward. The Nordic is highest, since it has the deepest and lowest nose.

In the curve of root-depth/apical depth ratio (Fig. 74) the Negro is in a class by itself, of course on account of its low root depth. The Nordic curve is generally the highest.

In the curves of nose width/interocular distance (Fig. 78) the Negro stands again, of course, above the others, while the Nordic curve is low, because of a high interocular distance in Nordic males of the LVDII series.

Of the curves of root-depth/interocular distance (Fig. 82) the Negro's lies at a low level on account of shallow root. The Nordic and Mediterranean curves lie not far apart.

To summarize: In racial differences in growth of the nose the Negro stands quite apart from the two European groups. That race shows the following traits: Wide nose,

deep salience, shallow root, high nasal index, low depth/width, high width/bizygomatic; low depth/height; low root/apical depth, high width/interocular distance; low root depth/interocular distance. That is, the nose of the mean American "Negro" is characterized by the following dimensions: It is wide, has large salience (but not large apical depth) and shallow root. Its outstanding nasal proportions are due to the exceptional dimensions. It is noteworthy that these differences are about as marked in Negro children of 6 years of age, or under, and indeed in fetuses, as in adults. In nasal height and apical depth the Negro does not differ from the Europeans.

As contrasted with the Mediterraneans the Nordics (in this study) have a lower nose, a slightly deeper nose root and a smaller nose width/interocular distance ratio.

c. *Individual growth and change.*—The averages that we have been considering fail to take into account the great variety of nasal dimensions even in the North European stock.

a. *Absolute measures.*—In general, the absolute changes of size with age are increases; sometimes linear, usually showing considerable deviation from linearity. Age changes in *nasal height* usually show increase in slope during adolescence corresponding to the spurt of body height as a whole. In some cases (Fig. 5, curve 3) no such spurt in nasal height has occurred up to 16 years. In other cases (Fig. 5, curves 4, 5) the rapid increase has ended by 17 years.

Nasal depth grows much as nasal height does, showing usually the same adolescent spurt. In colored children (Fig. 10, curves 7, 8) while the dimension is small, growth is at about the same rate as in whites. Among special cases (Fig. 13) No. 2 is a cretin whose nose depth began to increase greatly after 13 years of age. In the cases of 3 dwarfish girls, Nos. 3 and 6, growth in nasal depth made a sudden spurt at the 14th to 16th year. At the same time

in these girls the rate of growth of the body as a whole increased greatly.

Nasal width. This dimension grows with the greatest uniformity of all, though some slight deviations of slope from linearity are frequently found (Fig. 17: curves 3, 8, 9; Fig. 18: curves 6, 12). The growth of nasal width in four colored children is shown in Fig. 17, 1, and Fig. 18, 1, 2, 3, 4. In colored girls the dimension is relatively large at all the ages given but the slope of the growth curve is similar to that of white girls in Fig. 18; that of colored boys (Fig. 17) is exceptionally great. In the four dwarfs shown in Fig. 20, progress seems to be by fits and starts.

Nasal salient. The growth of this dimension in individuals is much more irregular than that of any other of our series. It is the only one in which there is a temporary decrease, as *e.g.* in Nos. 2, 6, 7 of Fig. 25. This is ascribed to the thickening of the upper lip with the development of the hair follicles. With this thickening the subnasion is thrust forward toward the apex of the nose. This reduction in nasal salient is less commonly found among girls (Fig. 24). Let us consider in detail the times of formation of the moustache in some of the boys in Fig. 25.

Curve 2, M.H., No. 2. A depression occurs at 15–16 years. At age 16 semiterminals (3 mm) first appeared on lip; he shaved before 19 years.

Curve 3, W.M., No. 8. Minimum of salience at 13th year. A marked slowing up of growth of salience at 16 years. Terminal hairs were first observed on upper lip at $16\frac{1}{3}$ years; short down only was there at $15\frac{1}{2}$ years.

Curve 4, I.J., No. 18. The data are rather inadequate. At $15\frac{10}{12}$ years his upper lip bore down 1.5 mm long; the nasal salient was 14.4 mm; at $16\frac{11}{12}$ there were semiterminals 3 mm long; nasal salient 15.5 mm.

Curve 5, F.W., No. 24. No marked depression but a slowing of increase of salience at 16th year. On upper lip, at $13\frac{7}{12}$ years, down 1.5 mm long; at $14\frac{9}{12}$ years, semiterminals 3 mm; at $15\frac{7}{12}$ years semiterminals 3 mm; at $16\frac{9}{12}$

years semiterminals 4 mm long. Terminals did not appear until about 19 years.

Curve 6, J.C., No. 10. Minimum of salience at 16 years. At 15 7/12, down 3 mm on upper lip; at 16 years, semiterminals 4 mm; at 17 8/12 terminal hairs well developed.

The facts of salient growth of other boys are given in Table 3.

TABLE 3

RELATION BETWEEN AGE OF CESSATION OF GROWTH OF NASAL SALIENT TO DEVELOPMENT OF HAIR ON THE UPPER LIP

Ages are given in years and months, separated by a colon. *d*, down; *s.t.*, semiterminal hairs, Nas. Sal., nasal salient.

No.	Initials							
7	A M	Ages: Nas Sal. Lip Hair	15:0 14 mm *d* 1 mm	16:0 16 mm *d* 1 mm	16:6 18 mm	17.0 16 mm *d* 3 mm	17.6 18 mm	18:0 17 mm *s.t.* 3 mm
23	F. B.	Ages: Nas. Sal Lip Hair	13:8 18 *d* 2 mm	14:8 18	15.8 21 *s t* 3–4 mm	16:8 19 (Shaves)	17:9 19	
25	H. M.	Ages: Nas. Sal. Lip Hair	14:10 18 *d* 1 mm	15.11 19 *d* 3 mm	17:0 16 *s.t.* 4 mm	18 0 18 *s t* 4 mm	19:0 19 Shaves	
28	F C	Ages: Nas. Sal. Lip Hair			17.5 17 *s.t* 3 mm	18.4 17 *s.t* 3 mm	19:4 17 *s t* 3–4 mm	
29	R. H.	Ages: Nas. Sal. Lip Hair	15.6 *d* 2	16:6 19 *s t.* 3	17:6 21 *s t.* 3	18.0 18 —	18:6 19 *s t.* 3	
40	A. B.	Ages. Nas. Sal. Lip Hair	13:11 14 *d* 1	14 11 15 *d* 2	15.11 16 *s t.* 4	16:11 15 *s t* 3	17:11 17 Shaves	
43	H M.	Ages: Nas Sal. Lip Hair	16:3 18 *d* 2	17 2 17	18:3 17 *s t* 3	19 3 20 *s.t* 4		
45	F C.	Ages: Nas. Sal. Lip Hair	16.5 21	17:6 16 *s t.* 4	18:6 16 Shaves	20 1 18 Shaves		

We may conclude from these data that the depression, or retardation, in the individual growth curve of nasal salient is frequently associated with the thickening of the upper lip, probably due to the development of the terminal hairs of the moustache. The development of the third molar may play a part.

Nasal root-depth. The curves of growth of this dimen-

sion are, for the most part, rather uniformly progressive; indeed, sometimes linear. As the growth in this dimension is associated with the approximation of the inner eye angles the speed of that growth may well vary with growth changes in other parts of the head.[1] It is, indeed, subject in males to a spurt of growth at 13 to 16 years; about the time of the male adolescent spurt.

β. Ratios.—A general review of individual curves of ratios shows a condition very different from that of the absolute measurements (excepting nasal salience).

Were proportions of the nasal dimensions constant at all ages the ratio lines would be horizontal ones through the age series. Examples approaching this condition are shown in Fig. 41 C in the nasal indices of the Mea. family and in Fig. 59 for the nasal ratios of three boys. On the other hand there may be great swings in the ratio as in depth/height of Fig. 69.

In some cases the age curves of ratios decrease regularly, in other cases they increase regularly, in others they are U-shaped, and in still others they are more or less wave-like (Fig. 39). These irregularities with age in the position of the ratio indicate that proportions of the nasal dimensions are changing with age.

In the case of the nasal index a decrease with age indicates that nasal bones and lateral nasal cartilage are growing in length faster than the wing cartilages in width, leading to a narrow nose (Fig. 36). When the nasal index increases with age (Fig. 37) the reverse is the case, and the

[1] Though at ages 12 to 18 the distance between inner eye angles is 60 to 100 per cent greater than nasal root depth, the latter increases two or three times as fast as the former. Obviously, the interocular distance is, as it were, being in part shoved up onto the nasal root; so that the remaining (lateral) parts of the interocular space increase very slowly. Example:

No. 16. Ages	12 : 9	13 : 9	14 : 10	15 : 10	16 : 11	18 : 10
Root depth (in mm)	15	16	15	16	18	21
Interocular distance (in mm)	31	31	32	33	32	33

bony growth of the maxilla, where it lines the *apertura piriformis,* advances slowly leaving the aperture wide. Some developmental factor ("gene") must inhibit growth in this region in certain families or human strains. In some cases the index is at first diminishing and then later increasing. Thus in curve No. 5 in Fig. 38 the height of the nose grows during 12 to 15 years at the rate of 1.7 mm p.a. while the width is growing at the rate of 0.0 mm p.a. At 17–18 years growth in height is proceeding at the rate of 1 mm p.a. while width is increasing about 2.5 mm p.a. The age period 16:2 to 18:1 is that of No. 5's spurt in stature and the stimulus to growth in stature seems in this case to have affected the width-growth factors more than the height-growth factors. One sees how sensitive the index is to changes in rate of growth of the dimensions involved in it.

Cases of practically linear changes in ratio are of interest. One such is given in Fig. 49 *H* for the depth/width ratio. At the bottom of the chart the absolute developmental changes of width (*Hw*) and depth (*Hd*) are shown. These increase uniformly and at different rates, so that the ratio line ascends rapidly and fairly uniformly. In Fig. 45 we have 3 lines of fairly uniform (linear) advance but at different rates: 4.5 mm, 3 mm, and 1.5 mm p.a. respectively.

Ratio age-curves that are concave below are not very common. Some of the most marked are shown by the salient/depth ratio (Fig. 64). Take for example No. 6 (J.C., No. 10). This nose is shown in Pl. VII, Fig. 14. Full data of salience and depth in his case are given in Table 4.

TABLE 4

DATA ON SALIENT/DEPTH RATIO OF J. C., NO. 10

Age in Years	9	10	11	12	13	14	15	16	17	18
Nasal salient .	13 0	13.5	14.0	16.0	17.0	18.0	17.0	16.0	15.0	18.0 mm
Nasal depth . .	24 0	24.0	25.0	27.0	28 0	26.5	26 0	25 5	27.0	34 0 mm
S/D	0.542	0 562	0.560	0.593	0.670	0.679	0 654	0.627	0.556	0.529

We have already seen that the salient in boys is apt to decrease during puberty, as occurs in this case. This case is rather unusual, however, in that the nasal depth increases slower than salient until about 14 years; and this is responsible for the rise of the S/D ratio to 14 years. A similar phenomenon is responsible for the rise in the graph of No. 8.

The question might be raised whether the down sloping line and the up sloping line are not merely parts, at different ages, of the line that is concave above. A comparison of Figs. 36 and 37 shows, however, that the curves of the two types may extend over the same age range, or from 7 to 17 years; and there is no reason to anticipate that after 17 or 18 years the nasal index of the boys in which it has been decreasing will thereupon increase. However, the important fact is that within the age range included in our study both of these distinct types make their appearance.

The difference in types of curves found in absolute measurements enforces the conclusion that the growth of any nasal dimension is at every stage under the control of genes. Such genes with their specific functions have, apparently, become incorporated in the organism and have come to persist because of their adaptive significance to the organism, or at least as a result of an inoccuous mutation. So it comes about that noses differ less in absolute size than in proportions of dimensions.

6. Different Types of Noses and How They Have Developed

The noses of the inmates of Letchworth Village show a great variability as is indicated in the accompanying photographic profiles (Pl. VII). In the white boys alone there are noses with bridges concave (No. 9), convex (No. 8), straight (No. 7) or slightly sigmoid (No. 6, humped nose or "Hoeckernase" of Hovorka). The apex has a small radius of curvature (6.5 mm in No. 8) or a large one (9.0 mm in Nos. 12, 13). The lower lateral margin of the wing is

on the same level with the septum (No. 9) or more or less bowed or carried convexly root-ward (No. 13). The sculpture of the wing adjacent to the sulcus is varied. It may be slightly or broadly extended laterally. The sulcus, marking the outer boundary of the wing, may be short and shallow (No. 14), or it may be deep and extended over an arc of 180° (No. 9). The union of the septum with the lip is varied. It may form an angle that is acute as in the Negro (Pl. IV, 8) and some whites (Pl. VII, 13). More frequently the junction is by a curve of radius of only 1 or 2 mm (Pl. VII, No. 9) or of 9 mm (Nos. 8 and 11).

The girls' series (Pl. VII) shows the same thing. Note the heavy wing of No. 5 (a family trait); the unobtrusive wing of No. 2; the high wing of No. 3 and the low wing of No. 1. Compare the straight bridge of No. 16 with the concave bridge of No. 15 (like her sister), or of Pl. IV, No. 4; the convex bridge of No. 5, or the slightly humped noses of Nos. 1 and 17. Compare the laterally exposed septum of No. 5 with the concealed septum of Nos. 3 and 15. The lip-septum angle is acute in Nos. 3 and 5, obtuse in Nos. 4 and 18. Salience in relation to depth is larger in Nos. 3 and 16, small in No. 17. The nose is high in Nos. 3 and 16, low in Nos. 5 and 18. The root is deep in No. 16, shallow in Nos. 1 and 2. The tip has a large radius of curvature in No. 15, a small one in Nos. 2 and 18.

Now all of these differences are developmental differences and from a study of other members of the family some are known to be family differences.

The genetical basis of nasal resemblance is well illustrated in the case of the B. and G. twins (Pl. V, Figs. 2 and 3. In the latter case it is demonstrated that the noses are alike because they have developed along the same developmental path in the same way (Fig. *B*, p. 196).

The mongoloid dwarf (Pl. IV, 5, 7) has a nose of infantile (almost anthropoid) quality. The root bridge is low, the wing portion developed to a slight or median degree.

Figure *C* is from a photograph of profile of two boys (E.H. and C.D.), showing the difference in form of adult nose. This difference in form was just as marked 11 years earlier as the curves indicate. In fact, if one traces the course of development of the dimensions of these noses from childhood it appears that they developed along somewhat different lines. Thus while nasal heights developed in parallel fashion, the development was at different levels in the two cases. The same is true of nasal depths. But the development of depth of bridge at root was different in the two cases; since the root ceased to grow in C.D. after 16 years, while it continued to grow in E.H. The final form of the nose is different because the course of development is different in the two boys. We say the form of the nose is inherited. What was really inherited was the course of development.

It has not been thought worth while to give the growth curves of the noses of all of the individuals shown on Pl. VII. Some of the individual growth curves are shown on some of the graphs, as indicated. The rule just given holds generally.

The changes in form of the nasal profile of the boys during the course of 10 years, more or less, is indicated on Pl. VIII, in which all profiles are given half natural size. Some interesting differences in mode of development appear. Thus in No. 14 the change in facial proportion, both above and below pronasion, is great; in No. 10 the part of the face above the subnasale has altered little while that below has changed much. On the other hand in No. 11 the part of the face below the subnasale has altered little while that above has changed considerably. Each face develops in familial fashion under the control of its special genes. Thus No. 10 has a huge mandible of the acromegalic type and we see that this developed during late adolescence. Similarly with No. 6 who has developed a Mussolini-like chin. It is certain that at birth these 9 boys, though they were already clearly distinguishable, were far more alike

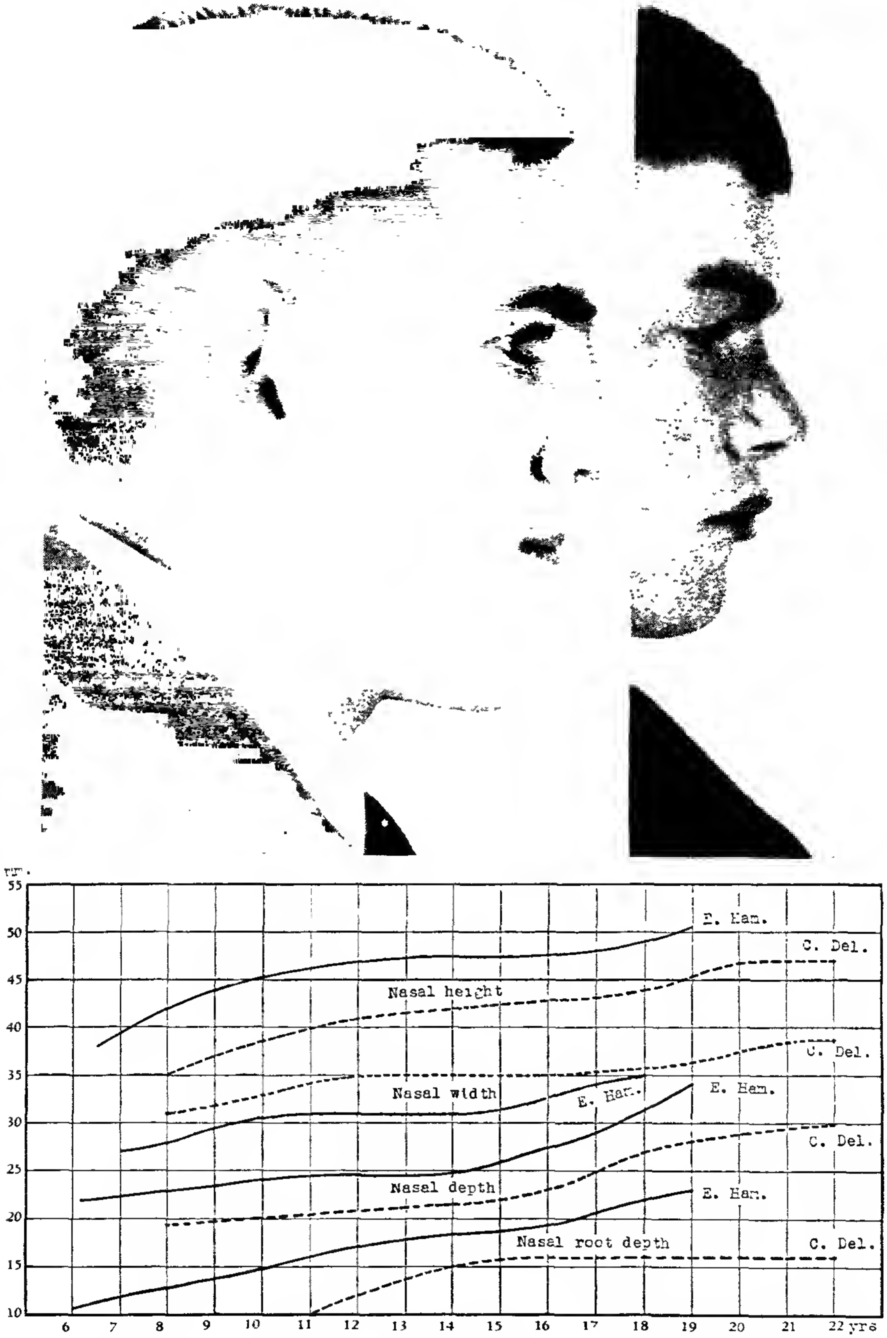

FIG. C. Profiles of Earl H. and Charles D. and the curves of growth in each of them of nasal height, nasal width, nasal depth and nasal root depth, between 6 and 22 years of age. Scale of millimeters at left.

than they are as adults. It is in the full working out or expression of the facial dimensions that family and individual differences become most completely expressed.

7. The Reliability of the Curves of Growth of Nasal Dimensions

Nasal dimensions are small dimensions so that any inaccuracy gives a proportionally large error. This fact has been appreciated by the author from the beginning and especial care was taken to secure accuracy. But with what result? Steggerda (Davenport, Steggerda and Drager, '34) measuring the same adult 50 times during a number of days, found for his measurements of nasal height a standard deviation of 1.57 mm; of nasal depth, $\sigma = 1.31$ mm; of nasal salient, $\sigma = 1.50$ mm; of root depth 1.65 mm; with coefficients of variation of 2.79, 4.74, 7.20 and 11.24 per cent respectively. These figures indicate a large proportional variation in these measurements. When Steggerda and the author ('34, p. 274, 275) measured the same eleven adults each twice, the following differences were obtained: i.e. quadratic means[1] of the differences obtained by the named observer divided by mean for measurement $\times$ 100 (cols. 1 and 2).

	1	2	Average Difference between Measurements of Observers 1 and 2
	Steggerda	Davenport	
Nasal height	4.42%	3.34%	+5.18 mm
Nasal width	3.34	3.71	+0.01
Nasal salient	6.71	6.28	+2.45
Nasal depth	6.88	7.59	−0.05
Root depth	1.59	2.14	−1.86

The large difference in nasal height is due to the different landmarks used by the two observers. I believe in my own measurements there is a much closer consistency.

[1] Quadratic mean is the square root of the difference between each pair of measurements squared and summed for all differences; then divided by the number of individuals measured.

The nasal salient is also difficult to define for reasons already referred to in the section on Methods. Nasal width and depth are dimensions that are fairly precise. Root depth is taken with intermediate precision.

The present study is based on sets of measurements taken on the same individual at more or less frequent intervals. The results of each measurement as made were plotted and a curve drawn through the points. Since several curves are drawn on one graph to save expense and space, the plotted points are not reproduced in the published charts. The final curve is smoothed conservatively, neglecting a point that is clearly very erratic.[1] The true location of the first (youngest) point of a curve is always the most uncertain. The curves as finally inked in for publication are believed to be the closest approximation to the truth. The original data on any curve will be preserved at the Eugenics Record Office and made available to inquirers.

8. The Racial Factor in Nasal Form

We have seen the great diversity of nose form among adult persons of European origin. If we look over the whole human race we are impressed by the great range of nose forms in different areas with considerable resemblance in any one, leading to the conception of racial types. Fig. *A* (p. 181) illustrates some of these. Even better, it has been possible to show in Pls. IX–XVIII a number of more or less characteristic faces of some of these types. These will serve to show racial differences, as well as individual differences inside the "race."

Chinese.—Pls. IX and X give frontal and profile views of a number of Chinese. The photographs on Pl. IX are of students of the Peking Union Medical College; those on Pl. X are of employes there. While, in general, the noses are characterized by shallow root and apex and large space

[1] In a few curves the original findings are plotted as points. See Figs. *B*, 19, 37, 39, 45, 53, 76, 84.

between inner eye angles, the series shows marked variations, as follows: The nose bridge is nearly straight in Pl. IX, Nos. 7, 10; Pl. X, No. 40. It is concave in Pl. IX, Nos. 26 and 39. It is convex in Pl. X, No. 16. It forms a small angle with the nasion-subnasale line in Pl. IX, No. 10, and Pl. X, Nos. 7, 40. The subnasale is only slightly in advance of the nasion-gnathion line in Pl. IX, Nos. 26, 7, 34, and Pl. X, Nos. 7, 16 and 40; more in advance in Pl. IX, Nos. 10, 19, 29; Pl. X, No. 15.

The apex of the nose is rounded in some cases, as in Pl. IX, Fig. 39. Perhaps the most characteristic form is like Pl. IX, Fig. 32. This type is seen slightly developed in Pl. IX, No. 7; very marked in Pl. X, No. 16.

The septal-nostril plane is usually somewhat elevated distally so that the nostrils are seen from in front as in Pl. IX 29, 39, and Pl. X, 2, 38. In other cases the plane is not elevated (Pl. IX, No. 7), or is even depressed distally (Pl. X, No. 13).

The form of the wing of the nose is variable. The axis parallel to the face is often nearly equal to that perpendicular thereto, correlated with slight nasal depth (Pl. IX, Figs. 10 and 39). However, the wing is sometimes depressed as in Pl. X, Figs. 7 and 16. In general the wings in the nose of the students have a well marked upper sulcus, more frequently than those of the employes; cf. the frontal aspects of Pl. IX, Nos. 10, 26, 39, with those of Pl. X, Nos. 7, 16, 38, 40.

The nostril margin of the wing is frequently concave toward the lip (Pl. IX, No. 7); sometimes the margin is notched, as in Pl. IX, No. 19, and Pl. X, No. 15, and this notched condition is much more striking and common than in whites.

The form of the septum where it joins the upper lip seems to be less variable than in Europeans. The commonest condition is that of a free margin of septum united with the convexly curved upper lip at an angle of 75° to 90° (Figs. 32, 34, 39). In other cases the lip is straight or

concave and the angle of union is 80° to 110° (Nos. 7, 29, 38). Rarely is the union a curve, as in No. 40.

The frontal view reveals a nose that in relation to height is broad. Other authors (Martin, '28, p. 552) have found a nasal index of 73 to 83 for adult Chinese in contrast with an index of about 70 in our series of European stock.

The ratio of salience to nasal depth is, in general, high (Figs. 10, 26, 39, 7). Probably it will prove to be on the average around 66 per cent as compared with 55 to 57 per cent in European stock. A ratio of 75 per cent is not uncommon (Pl. X, Figs. 2, 20).

To summarize, the outer nose of the Chinese in the Peking region is characterized (as compared with Europeans) by these characters: Nasal depth is low from root to tip. Interocular distance is wide. Bridge line forms a small angle with nasion-subnasale line. Form of bridge-apex outline is helicoid. The wing diameters are equal. Nostril margin is concave below or notched. The angle of union of septum and upper lip is acute. Nose is wide. Ratio of salience to depth is high, about 66 per cent.[1] In general the North Chinese nose is a less pronounced feature than the North European.

Asiatic Indians.—From such photographs as I have been able to consult the Oraons, of Central India, seem to have a helicoid bridge outline, septum extends below nostril margin, wing somewhat depressed; nasal width medium to large (Pl. XI, 1–7). Portraits of the Gondide race given by v. Eickstedt ('31) show nearly straight bridge outline. The Palaungs of Burma have great nasal width, large nasal wings and uptilted nostril plane (v. Eickstedt, '28).

Some Bengalese (Pl. XI, 12–14, Pl. XII, Pl. XIII, 2, 3) have straight to helicoid nose bridge outline. The nasal width is greater than of Europeans. There are large nostrils and horizontally elongated wings. The septal-labial angle is acute, 85 to 75 degrees.

[1] Good photographs, 200 × 135 mm. of Chinese heads have been published by Birkner ('05). These show many characters cited above.

The Dravidians (Pl. XIV) retain some Neanderthaloid nasal traits. The nasal height is medium to low; the nasal width is very great, approaching that of the Negro (Pl. IV, 6). The bridge outline is more or less concave. The wings are well developed. The septal-labial angle is acute.

Some of the Jewish Cochin (Madras) hybrids have very convex noses (Pl. XV).

The Todas of Southern India have mostly noses that are of medium height, wider than those of Europeans, bridge straight to slightly bowed, the wings large (Marshall, 1874).

The Senoi of the Malay peninsula have low wide noses, well developed wings with a low position of attachment to cheek so that nasal apex is somewhat elevated (Martin, '28, p. 565).

Polynesians.—The nose approaches the European in height, but is wider. The bridge is usually slightly bowed.

Amerindians (Pl. XVI).—The nose is usually high but of varied bridge contour. Apparently straight in the Southern Patagonians (Stratz, '22, p. 315); bowed in the Indians of Malto Grasso Brazil (Petrullo, '32) and in many of the Mayas, Pl. XVII (Steggerda, '32, Pl. V), frequently aquiline in the tribes of middle North America (Pl. XVI). The angle between septum and upper lip is usually acute to rectangular, the upper lip being full and sometimes protruding. The width of the nose, rarely extreme, is varied in different peoples.

Africa.—The Negro is variable. The following are generalizations that are common to many tribes. Nose low, extremely wide. The root is shallow, the contour of the bridge is straight or slightly concave; but it is sometimes slightly convex, even aquiline or wavy. The most constant feature is an acute angle between septum and upper lip, due to a fullness and protrusion of the lip (Weninger, '27). In some of the South Africans (Pl. XVIII, 1, 2) the nasal type of the Negro reaches an extreme. In the American

Negro (Pl. IV, 6, 8) the nasal form approaches the white type because of hybridization.

The north Africans, Arabs, Bedouins and allies have mostly very high and narrow noses, frequently with convex, bowed bridge contour.

Banda Sec region. Timor.—The nasal height is medium; the width great but less than in French West Africans. The wings are prominent with deep upper wing sulcus, but usually elliptical in outline. The bridge contour is mostly straight or slightly convex (Bijlmer, '29). The noses of the hybridized inhabitants of Kisar are intermediate between those of Timor and Europe (Rodenwaldt, '27, pp. 355–371).

Java.—The aborigenes of Java have (following Nyèssen, '29a) noses mostly of medium height, medium width or above, shallow root and apex. Salience is about 50 per cent of nasal depth. The outline of the bridge is nearly straight or slightly concave. The nostril plane is frequently uptilted (Pl. 33, 34) and the septum-lip angle varies from 70° to 100°. The most striking feature of the Javanese nose is its shallowness, in extreme cases of the "mongolian dwarf" type (Nyèssen, '29).

The *Papuans* show some types like those of the Dutch East Indies; others approaching the aboriginal Australians. These Australians (Pl. XVIII, 3–6) are characterized by low noses with shallow root. The apex is of intermediate depth. The bridge contour is rounded, sometimes slightly concave at middle of bridge. The nose is broad, the wings well developed and the outline of the wings between circular and elliptical. The septum-lip angle is acute (around 80°) and the plane of the nostrils is commonly uptilted so that the nostrils show in face view.

European.—These are perhaps fairly represented by the profiles (Fig. *A*, & Pl. VII). There are, however, certain extreme types that should be mentioned. Thus the Albanians have high, deep, narrow noses, with mostly convex bridge. The salience ratio is high to medium. The wings

are shallow and elliptical in outline. The angle between septum and upper lip is mostly 90° or more (Weninger, '34).

The East Baltic type has nose of medium height and width, but rather shallow. The contour of bridge is concave, the nostril plane is uptilted so that nostrils are rather conspicuous in frontal view.

The Lapps have noses of medium height, shallow root, deep apex and concave bridge. The wings are mostly not prominent. The septum often passes into the upper lip by a curved line.

9. The Genetic Factors in Nose Development

The form of the adult nose is determined by a number of factors, intrinsic and extrinsic to the germ plasm. Of the latter, disease, such as yields the "syphilitic nose" and accident, such as is responsible for the "football nose" or "prize fighter nose," are effective but fortunately not common. The usual differential factors determining nasal form are intrinsic-germinal.

How many germinal factors or genes can be recognized? Let us consider some of the obvious ones.

Factors determining acceleration (or inhibition) in length and in width of the nasal bones (Pl. III, 5, *a*).

Factors for the frontal process of the maxilla, which is derived as part of the maxillary center of ossification of the maxilla. The form and size of the frontal process must influence somewhat the protuberance or elevation of the nasal bones and the curvature of their medial border. This in turn influences the form of the bridge at root and one or two centimeters below the root. The shape of the nose must be largely influenced by the shape of the nasal notch or lateral boundary of the piriform aperture, which in turn is a part of the "maxillary" center of ossification. The form and size of the region of the anterior nasal space and the alveolar process of the premaxilla exert an influence on the shape of the nose.

Factors determining the form of the nasal cartilages both lateral, wing and septal cartilage. The two former start development as one piece, as in all Primates, but become separated in the seventh fetal month (Wen, '30, p. 122). The septum starts as a plate of mesenchyme which later unites with the lateral ("roof") cartilage and doubtless helps determine the shape of the latter. Apart from the septum the form of lateral cartilage and wing cartilages must be determined by growth controlling factors (genes).

Factors determining the rounding off of the nasal surface. These include the subepidermal tissues, with their fat, and the thin muscles of the nose; the procerus over the nasal bones, the nasalis with the two parts, the caput angulare of the quadratus labii superioris, the dilatores naris, anterior and posterior, and the depressor septic nasi. But not all of the muscles are present in every one.

If we assume that only 10 independent genes are involved in the shape of the nose and of these, say, any five may be absent or recessive, we would have the combination given by 10 things taken 5 at a time, or 252. Thus a quarter of a thousand different types of noses would be accounted for. If, as is probable, multiple allelomorphs are involved the number of combinations might run into the millions.

But in addition to the genes that act directly in moulding the form of the nose there is the influence of other genes that affect the growth of the nose only indirectly, such as those that affect the activity of endocrine glands. Thus we have seen how some of the nasal dimensions suddenly increase at the time of the spurt of growth of the body as a whole, ordinarily ascribed to the secretions of the anterior pituitary. Factors that influence the structure of bone generally in body, such as chondrodystrophy, osteogenesis imperfectæ, osteopsathyrosis, affect the growth of bones and cartilages of the nose. We have seen how in Mongoloid dwarfs the growth of the nose is affected.

In view of all of these varying genetic factors affecting the form of the nose we can understand why one rarely

finds two noses alike and one is impressed by the similarity shown by noses of identical twins (Pl. V).

How can such an identity of form of nose be brought about? Fig. *B* shows for a few of the dimensions the practical identity of their growth. This identity means that each growth center begins to increase at the same time and the rate of growth of each is the same. If in the twins the nasal depths at apex show at each age the same size, then the form of the maxilla and its aperatura piriformis, the nasal septum, the wing cartilages with their two angularly divergent crura, the muscles, subdermal fat and connective tissue, must grow equally. Finally, the growth-promoting endocrine glands must act with equal efficiency. Thus of nasal bones, maxillæ, cartilages, skin, endocrines, the developmental processes (which by chance would all be twice the same only once in millions of times) are here indistinguishable in their collective development. Even though the food ingested is not always the same, that makes little difference, for the developing tissues take only what their specific chemical factories can use and reject the rest.[1]

A similar collective development requires that, starting from the same centers, identical cells, under the control of identical genes, shall start development at the same time and place and proceed at the same rate toward their predestined end. And the activity of each of these centers shall be so timed and placed with reference to the others concerned, and shall be so coordinated with the period of activity of distant activating factors as to produce not merely a harmonious whole but one which will be just like that proceeding in another individual who, in post partum life, is physically wholly independent of it.

In contrast to identical twins, biovular twins, and other members of a fraternity, rarely show identical developmental curves. But they do usually show resemblance. In some cases the resemblance is in the general form of the curves and the eventual proportions attained, but the

[1] Incidentally the food offered the Letchworth children is the same for all.

time element is not the same; the similar proportions are achieved at different ages (Figs. 6, 19, 27, 41 *C*, 51, 71). Again, the curves may be timed similarly at certain ages but differ somewhat in form: Figs. 12, 32 (1, 3), 50 (*E, M*). In both cases one or more developmental factors (gene-cytoplasmic reactions) are different in the various members of the family. But some similar features are usually found in members of a family. The family sometimes falls into two groups with reference to a particular nasal dimension, suggesting Mendelian segregation.

There is also racial resemblance. In some of our figures the members of two races fall into two widely separated groups, the separation being made along racial lines (Figs. 10, 17, 18, 38, 39 (1, 2, 4), 59, 60). The separating difference is often so great quantitatively as to be qualitative. Nearly all of the dimensions are differentiated, but they may not all be. Thus, nasal height is about the same in Negroes as in Europeans (Figs. 3, 5). Also, all of the well developed full-blooded members of the race will have the racial differential, though in various degrees of expression. Obviously these racial differentiating characters are due to special gene-cytoplasmic reactions or perhaps to extreme allelomorphs of a series of genes that produce different degrees of a trait.

The question is sometimes raised as to the influence of diverse environments in producing different types of noses. There would seem to be two possibilities. First, some particular mutations may be more favorable for certain particular environments than others and so come to be conserved in them. Secondly, it is possible that extreme conditions, regions of unusual ultra violet radiation, or of high temperature, may accelerate the mutation process; but this would tend to heighten variability in all dimensions and organs of the body; and a selection of the viables (including adaptation to environments) must act here also.

Finally, a word may be said about the striking phenomena of recapitulation shown in the development of the

nose as a region of the face. The beginnings of the nose are strictly lateral as in the fishes and during ontogeny move to the anterior position that they have in mammals. The cartilaginous elements of the nose are laid down as a pair of tubes as in all primates. Later, this tube breaks into the posterior (lateral cartilage) and anterior (wing cartilage) segments as in the higher primates. In an eight month old fetus usually only the apex of the nose bridge has been elevated, and this condition accords roughly with what we see in anthropoids (Pl. I). Then the root, as the human trait, deepens during childhood. The necessity of ontogeny recapitulating phylogeny has often been pointed out. There is no other way of reaching the goal of the adult individual but by going along the same old path that mammals, primates, anthropoids, have always taken. Just what determines the sequence of development of the differences is, of course, one of the great problems of development. Possibly the genes mutate during ontogeny as they have mutated during phylogeny. More probably the ontogenetic cytoplasm is being changed by gene-cytoplasmic reaction during development so that the nature of the new or later reactions depends upon the nature of the preceding reactions. The formation of the cartilaginous tube of the nose is one process; after this has proceeded sufficiently a division occurs. Only after this division are the two pieces in a position to develop in their special fashions—the distal piece into the alar cartilages supporting the tip of the nose; the proximal piece forming an ever steeper roof-angle and thus elevating the bridge of the nose. The distal piece develops in the human child differently from the proximal piece, probably because the cytoplasm of the proximal cartilage cells is different from the cytoplasm of the distal cartilage cells and so the genes of the distal cartilage nuclei do a different morphological job than those of the proximal nuclei.

E. SUMMARY OF CONCLUSIONS

The problem is how are the size and form of the human external nose determined; what are the laws of its increase and attainments of its eventual proportions? In answering this question use was made of our knowledge of the anatomical elements of the outer nose; and of their embryonic development. Then a series of measurements on fetuses, on infants and on children from 3 to 20 years was made. Also on individual children the nose was measured during from 5 to 15 years. The data discussed are grouped both in masses, giving size-age curves, and in individual, "longitudinal," series.

The data considered consist of 5 absolute dimensions and 8 ratios. Nasal height increases *pari passu* with stature and attains a greater size in boys than in girls. The individual curves of growth of nasal height show an adolescent spurt correlated with that of stature. The individual curves all run upward with age, but do not run parallel. Some dimensions stop growing early while others continue a vigorous growth. And in the different races the growth is different. In brothers the curves of growth are, typically, parallel but located at different levels, while in identical twins the curves are practically identical.

The growth of nasal depth follows a segment of the sigmoid curve. The nose in dwarfs and cretins is at first a shallow one. Some of our dwarfs, independent of treatment, show a spurt of growth of nose at a retarded adolescence and a treated cretin responds by deepening of the nose.

The nasal width grows rapidly before birth corresponding to its early development in phylogeny. This dimension in Negroes continues from birth its precocious prenatal size. Nasal width has a large absolute range of size from 33 to 44 mm. In individuals its growth almost ceases at about 16–18 years, but may continue to a much later period.

The growth of the nasal salient nearly ceases temporarily at about 1 year, probably due to development of the

maxillæ and tooth germs. This curve has a smoother growth in girls than in boys, since in the latter the curve is depressed by the thickening of the maxilla due to development of the permanent incisors and later by the thickening of the skin of the lips as hair follicles enlarge with the growth of a moustache. Accordingly, the curve of nasal salient in the male is very complex.

Nasal depth at root is late in developing as it is phylogenetically a late acquisition.

The ratios of nasal dimensions often develop in complex fashion. The width-height ratio diminishes rapidly prenatally from a mean of 115 per cent to 98, to reach a mean of 67 at maturity. Four types can be distinguished of growth curves between ages 6 and 18; increasing, decreasing, U-shaped, and irregular. There is a sort of "struggle" between the vertical and horizontal dimensions during development, resulting, nevertheless, in family resemblance. Three indices show a reversal of slope immediately at or shortly after birth.

There is a wide-spread sexual difference in nose size and proportions. This is largely due to the smaller size of the body of the female, which determines a relatively smaller nose and to the earlier differentiation of the nose in the female corresponding to the precocious acquisition of maturity.

The feebleminded tend to have a nose that is exceptionally deep at apex and root. The mongoloid dwarf has a low, shallow, narrow, unprominent nose.

While the difference between the nose of the Mediterraneans and Nordics is nowhere great, that between Negroes and Europeans is striking in the great breadth and low root of the former. An examination of photographs of the faces of a number of Chinese, Asiatic Indians, Polynesians, Amerindians, Negroes, inhabitants of the East Indian archipelago, Australians as well as Europeans reveals a great racial diversity in size and proportions of the nose.

The diversity of nose form suggests the presence of nu-

merous genes, each with multiple allelomorphs. To the vast number of possible combinations of these genes has to be added the influence of other genes, including those that affect the activity of endocrine glands with their growth-stimulating hormones.

The different development-controlling factors come into function at different times and places and at varying rates. Some of them function for a time and then disappear from the scene of action. The coming into function of a new gene may well repress the activity of another, as happens in the field of enzymes.

To summarize: Principal cases of recapitulation shown in the development of nose, apart from general increase in dimensions, are: Sinking of salient in the increasing mass of the maxilla in first two post-natal years. Great width of nose in relation to height at first, with its rapid relative decrease to 3 years post-natal. Shallow, broad nose as in apes at first steadily increasing in relation to depth. Early great width between alæ compared with face width, as in anthropoids; rapid reduction of this ratio. Early shallow salience compared with depth as in anthropoids; rapid increase of the ratio till first post-natal year; then varying ratio depending on periodic spurts of enlargement of maxilla. A low nasal depth compared with height at first, as in anthropoids, followed in prenatal life by a rapid relative increase in depth; thereafter a "race" between the two dimensions, with varying advantage. The nasal root is late in developing in both ontogeny and phylogeny; as it develops it draws the eyes, which are at first far apart as in apes, nearer together, to produce the human condition. The relative approach of the eyes, from the anthropoid condition, causes the nasal width to increase rapidly relatively to face width till the end of the first post-natal year. After that the relative change in these horizontal dimensions is slow.

The ontogenetic development of the nose follows in the steps of phylogeny since the later ontogenetic steps are

built upon earlier ones just as the later phylogenetic stages are built upon earlier ones.

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PLATE I

FIG. 1. Full face view of female gorilla. Courtesy New York Zoological Society.

FIG. 2. Profile of same gorilla showing wing and slight development of bridge. Courtesy New York Zoological Society.

FIG. 3. Full face view of female orangutan. Courtesy New York Zoological Society.

FIG. 4. Full face view of chimpanzee. Courtesy New York Zoological Society.

PLATE I

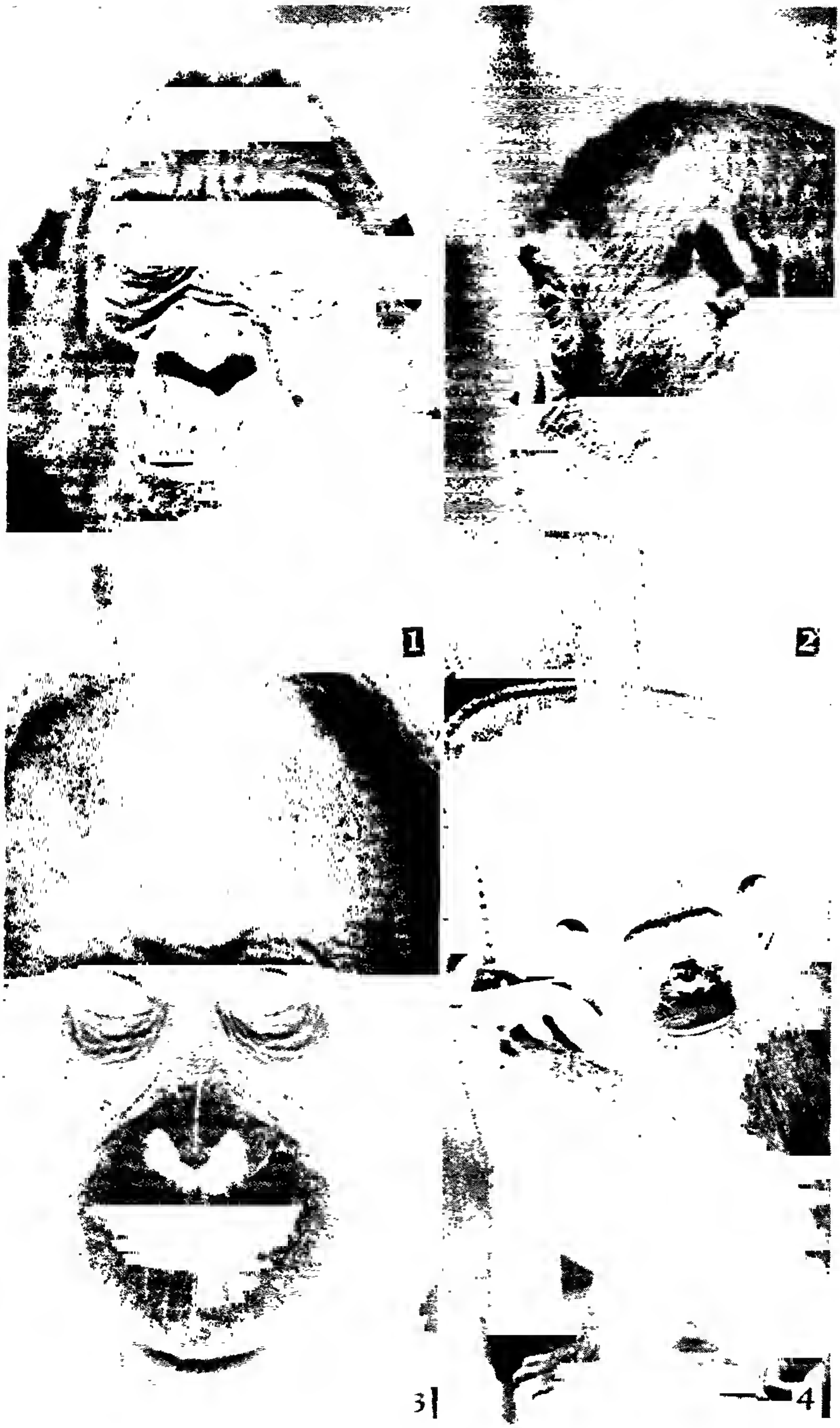

PLATE II

FIGS. 1 to 6. Photographs of female gorilla, Janet Penserosa, at New York Zoological Park, showing different views of nose and profile, illustrating the use of the nose in expressing emotions. No. 6 shows sweat, in the form of white spots, on the nose, demonstrating the activity of the sweat glands on the face of this animal.

Photographs made by Dr. Henry H. Russell by whose courtesy these photographs, of which he holds the copyrights, are reproduced.

PLATE II

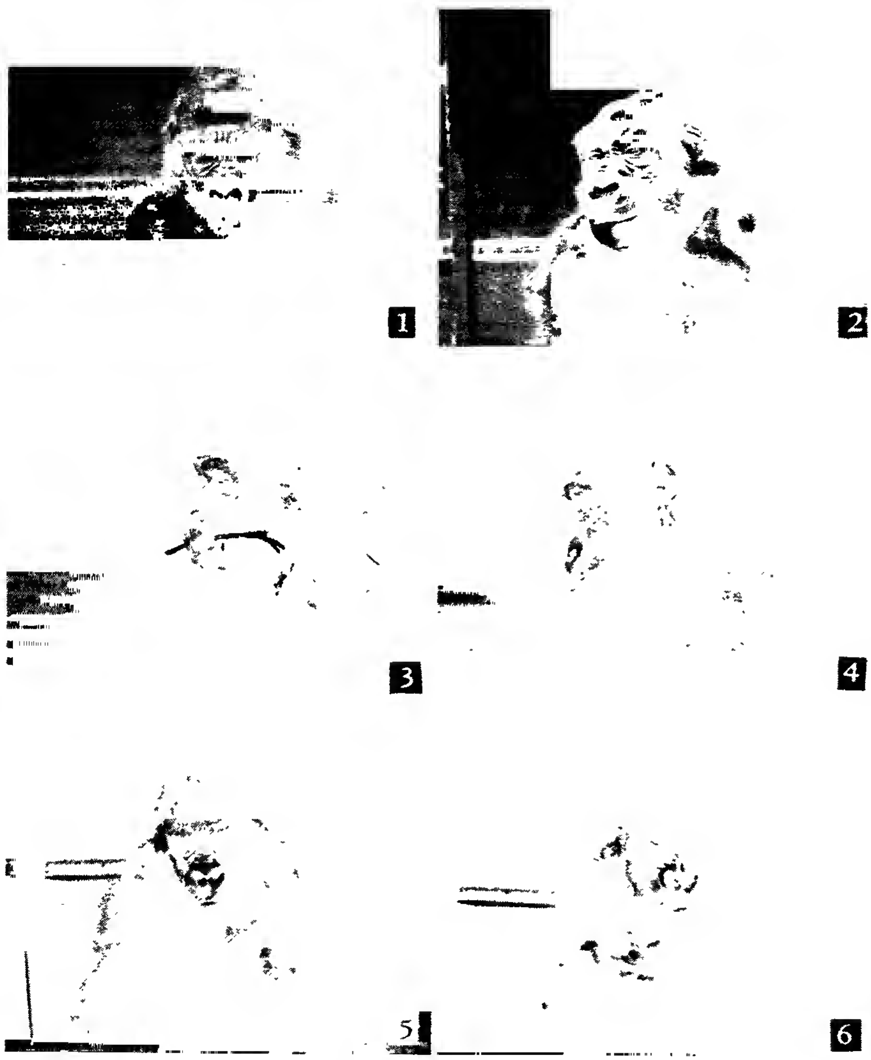

PLATE III

FIG. 1. Full face view of three children from Washington, D. C., of Old American stock. Ages about 8, 2 and 5 years.

FIG. 2. Profile of French West African Negro of Bambara stock, from Weninger, 1927, taf. 3, showing acute angle between septum and upper lip.

FIG. 3. Outline of profile of Norwegian from Walle. Bryn, Nordische Mensch. p. 107, showing obtuse angle between septum and upper lip.

FIG. 4. Profile of the bony and cartilaginous skeleton of the outer nose. *a*, nasal bone; *b*, margin of maxillary bone; *c*, lateral nasal cartilage, or roof cartilage; *d'*, lateral crus of greater alar cartilage, *d''*, medial crus of greater alar catilage; *e*, sesamoid cartilages; *f*, septal cartilage; *g*, naries; *h*, nasal vestibule; *n*, nasion.

FIG. 5. Dorsal view of bony and cartilaginous skeleton of the outer nose. Letters as above.

FIG. 6. View of outer nose perpendicular to plane of nares. Nasal cartilages in relation to external nares.

PLATE III

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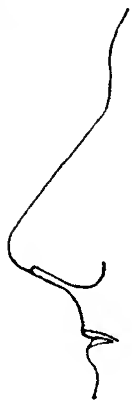

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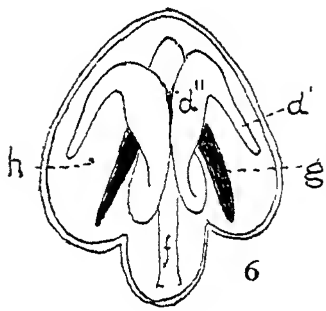

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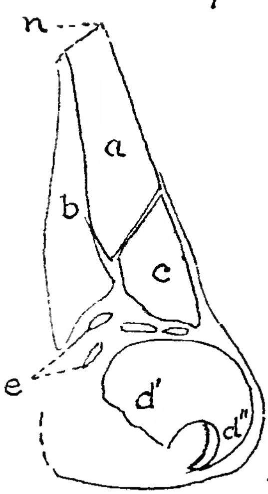

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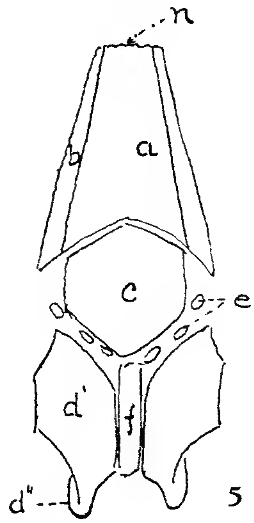

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PLATE IV

FIG. 1. Full face of C.A. LVD_{II}, No. 52, showing narrow nose.
FIG. 2. Full face of M.H. LVD_{II}, No. 51, showing wide nose.
FIG. 3. Profile of same girl as is shown in Fig. 1, straight bridged nose.
FIG. 4. Profile of same girl as is shown in Fig. 2, nose low and bridge concave.
FIG. 5. Mongoloid dwarf, R.B., full face.
FIG. 6. American Negro boy, L.B., full face.
FIG. 7. Mongoloid dwarf, same as seen in Fig. 5, profile.
FIG. 8. American Negro boy, same as in Fig. 6, profile.

PLATE IV

PLATE V

FIG. 1. A pair of identical twins, seen in full face. ERO No. A: 0538–902.

FIG. 2. Profiles of identical twins of the B. family. ERO No. A: 0538–346.

FIG. 3. Profiles of identical twins of the G. family. L.V.

PLATE V

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PLATE VI

FIGS. 1, 2, 3. Photographs of models of fetuses showing frontal view of the face in the second fetal month. 1, the mouth is seen lying between the mandibular process below and the maxillary process above, the latter being still paired. The nasal pits are seen as dark spaces right and left above. Behind, lateral to them, the lateral nasal processes. 2, the nasal pits have approached each other near the middle; the lateral processes appear just lateral to them. 3, the nasal pits are now close together. The lateral processes are wings lateral to them; the groove above them indicates the position of the root of the nose. Contributed by Dr. George L. Streeter.

PLATE VI

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PLATE VIA

Photograph of four living fetuses, supplied by Professor Davenport Hooker of the Medical School of the University of Pittsburgh. A, fetus of 8½ weeks; B, 11½ weeks; C, 14 weeks; D, 18½ weeks. These show the rise of the nasal apex with its nostrils and, later, the rise of the root of the nose coincident with the retreat of the frontal protuberance of the cranium.

PLATE VIa

A B C D

PLATE VII

Profiles of some of the children measured. FIGS. 1–5, five girls of the LVD_{II} series. 1, E.B., No. 7; 2, N.C., No. 27; 3, P.S., No. 105; 4, E.P., No. 86; 5, A.G., No. 48.

FIGS. 6–11, profiles of six boys of the LVD_I series. 6, J. B., No. 54; 7, H.M., No. 43; 8, W.M., No. 8; 9, M.H., No. 2; 10, M.G., No. 95; 11, T.V., No. 65

FIGS. 12–14, profiles of three boys of the LVD_I series. 12, G.W., No. 13; 13, F.W., brother of the foregoing; 14, J.C., No. 10.

Profiles of four girls of the LVD_{II} series. FIGS. 15, M.K., No. 60; 16, E.A., No. 124; 17, H.C., No. 21; 18, B.C., No. 25.

PLATE VII

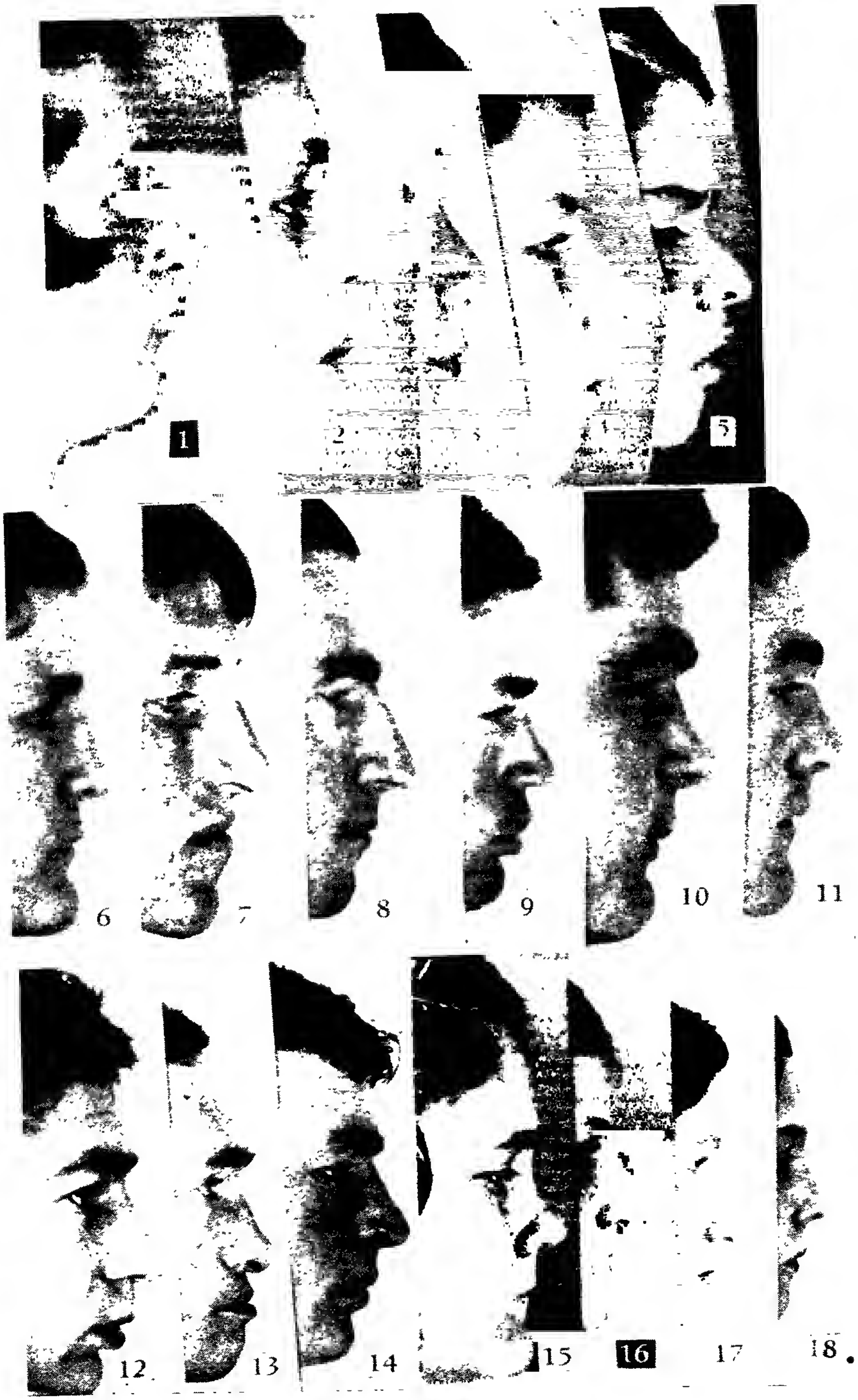

PLATE VIII

Outlines of faces of nine boys of the LVD_I series. The numbers correspond with those of Pl. VII. The dotted line is based on the earliest; the full line on the latest measurements. The ages are as follows.

No. 6.	6–19 years	No. 7.	10–20 years	No. 8.	7–17 years
9.	7–20 years	10.	10–19 years	11.	9–19 years
12.	12–17 years	13.	10–19 years	14.	8–18 years

PLATE VIII

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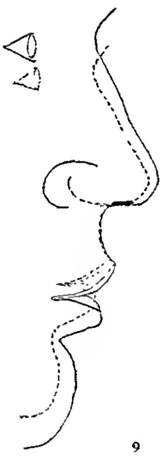

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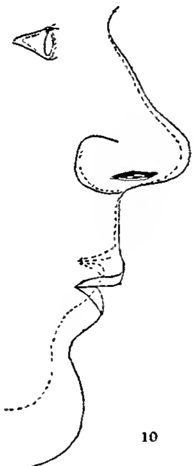

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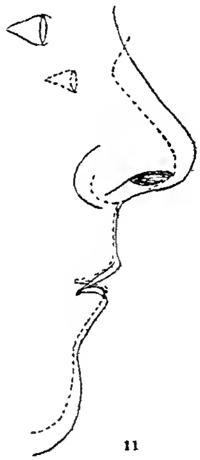

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PLATE IX

Frontal and lateral views of eight Chinese medical students at Peking Union Medical College. Kindness of A. B. Droogleever Fortuyn.

PLATE IX

PLATE X

Frontal and lateral views of eight employes of Chinese origin of the Peking Union Medical College. Kindness of A. B. Droogleever Fortuyn.

PLATE X

PLATE XI

FIGS. 1. Oraon (Central India, Chota Nagpur), female. Full face female child one year old. 2. Profile of same child. 3. Oraon aboriginal child, two years old, profile. 4, 5. Oraon aboriginal, male, negro type, full face and profile. 6, 7. Oraon aboriginal, male, full face and profile. 8, 9. Orya Kayastha Hindu, full face and profile. 10, 11. Hindu Sindi from Karachi caste, full face and profile. 12, 13. Contrasting types in Bengali. Cultivation caste of Mahisya. 14. Bengali. Hindu. Mahisya caste. Courtesy of Mrs. E. W. E. Macfarlane.

PLATE XI

PLATE XII

FIGS. 1, Bengali Hindu. Mahisya caste. 2, Bengali Brahmins, daughter No. 1 and mother No. 2. 3. Bengali Brahmins. Male, son of No. 2 above and brother of No. 1. 4, Profiles of same. 5 and 6, Bengali Brahmins, frontal and profile views, ages (left to right) 5, 6, 7 years. 5 year old is daughter of No. 1 in Fig. 2; 6 and 7 year olds are children of the pair shown in Figs. 3 and 4. Kindness of Mrs. E. W. E. Macfarlane.

PLATE XII

PLATE XIII

FIGS. 1. Mixed Sudra caste children. 2, 3. Adult Sikh from Punjab-Budge, Bengal. 4, 5. Left: Nepali Chattin caste, age 12. Right: Tepcha Mongoloid aborigines of E. Himalas, age 12. 6, 7. Mahisya Bengali Hindu, front and profile. 8. Tibetal profile: Yatung. Courtesy of Mrs. E. W. E. Macfarlane.

PLATE XIII

PLATE XIV

FIGS. 1. Kanikka with ship bow. Kadiyar Dam. Travancore. 2, 3. Dravidian, full face and profile (Madras Govt. Museum). 4. 5. 6 and 7. Dravidians, full face and profile views (Madras Govt. Museum). Courtesy of Mrs. E. W. E. Macfarlane.

PLATE XIV

PLATE XV

Figs. 1, 2, Cochin white Jew, aged 24. Full face and profile. 3, Cochin black Jew. Probably Malayali × Arabian Jews in past. 4, Cochin white Jew. 5, White Jew of Cochin (from Bagdad 100 years ago). 6, Cochin white Jewess, 24 years of age. 7, profile of No. 6. Courtesy of Mrs. E. W. E. Macfarlane.

PLATE XV


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PLATE XVI

Figures of North American Indians furnished by the Bureau of Ethnology, Washington, D. C., originals on deposit at ERO.

FIG. 1. Nearly profile view of J.J.N. of Santa Clara Tribe (1898). No. 1954–0–2.

FIG. 2. Nearly full face view of "Buffalo Goad," Wichita Tribe, prior to 1877. No. 1310.b.

FIG. 3. Approximately profile view of the same individual as No. 2.

FIG. 4. Profile view of Bannock Chief (1912). Shoshonean Family. No. 1708.b.

FIG. 5. Full face view of same individual as No. 4. No. 1708.a.

PLATE XVI

PLATE XVII

Portraits of Maya Indians of central Yucatan. Noteworthy are the low septum in the six faces, curved bridges, reminiscent of a bas relief on the temples, and the low and rather broad (triangular) form of the nose viewed in front. Photographs supplied through the courtesy of Dr. Morris Steggerda.

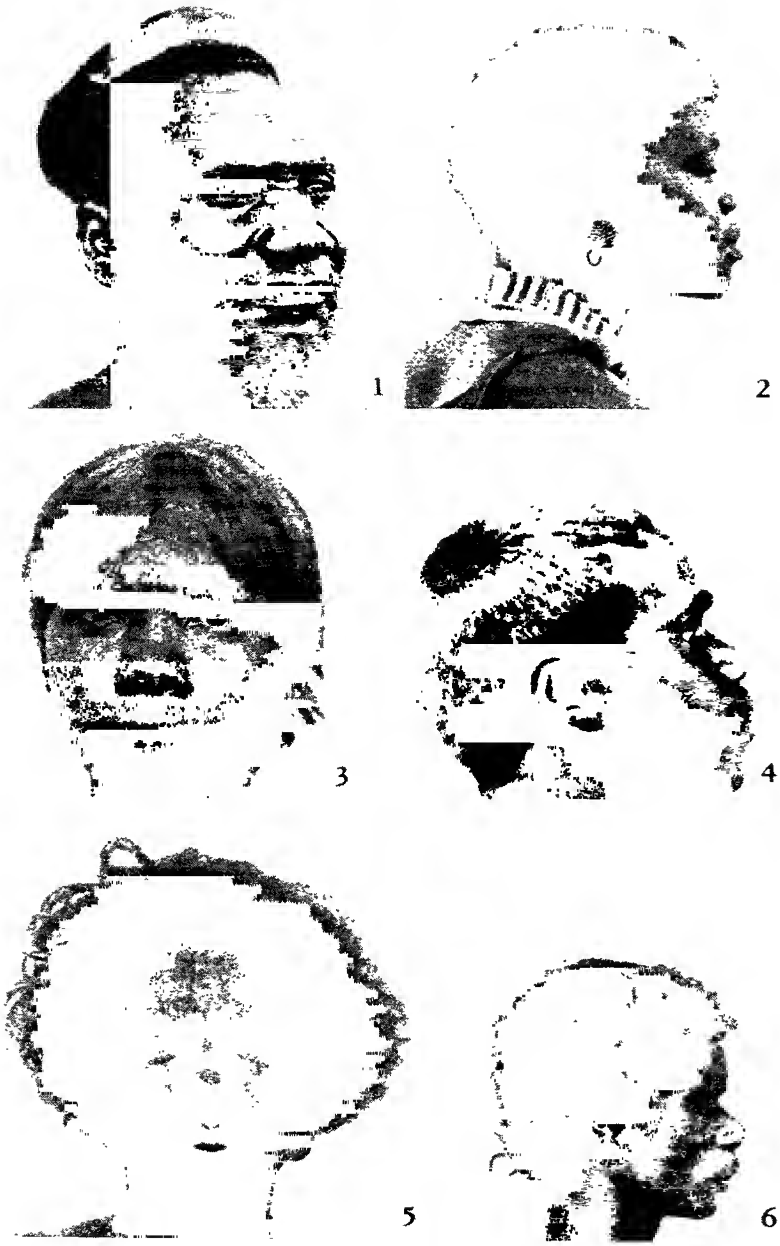
1. Andrea Cen, 1937.
2. Adelaida Chan, 1937.
3. Adolfo Kumul, 1935.
4. José Ceme, 1931.
5. Abroncio Dzib, 1932.
6. Facundo Ceme (brother of No. 4), 1932.

PLATE XVII

PLATE XVIII

FIGS. 1. Negro from North Transvaal, courtesy of Mr. A. M. Duggan-Cronin. 2. Shangaan Portuguese woman of East Africa. 3, 4, 5, 6. Heads of Australian aboriginals from photographs copyrighted by Kerry Co., Sidney, Australia.

PLATE XVIII



MEASLES AND SCARLET FEVER IN PROVIDENCE, R. I., 1929–1934 WITH RESPECT TO AGE AND SIZE OF FAMILY

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Abstract

The Providence, R. I., records for measles and scarlet fever for the years 1929–1934 inclusive have been worked up for comparison with the earlier findings of Dr. Chapin and Dr. Pope, but have been subtabulated according to size of family. It is shown that the percent age incidence of the diseases is different in families of different sizes. The age distribution of the children in families of different sizes is itself very different as is shown from the tabulation of a sample of the special Civil Works Administration-Federal Emergency Relief Administration census of Boston in January 1934, and these results are used to obtain rates of incidence by age and by size of family. The rate picture is very different from that of the percentage distribution in that the rates are relatively higher at the early ages in the larger families. Special attention is paid to the two-child family. Greenwood's mathematical theory for the spread of measles in the family is carefully examined for families of more than two susceptibles and it is shown that his theory seems not to be substantiated in that the attack rate on susceptibles who do not get their infection from the primary case but get it from a secondary case is markedly lower than for those who get it from the primary case. The first half and the second half of the great measles epidemic of 1931–2 are compared to show that the incidence by size of family differed in the two halves. Finally cases of measles are tabulated for Boston by Wards for 1916–1936 to show that the coefficient of inter-ward variation by years is of about the same magnitude as the coefficient of inter-year variation by wards.

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1. *Introduction.*—In 1925 Chapin [1] discussed measles in Providence for various periods of years, of which the longest was from 1858–1923; about the same time Pope [2] gave a similar discussion of scarlet fever in the same city. We propose to discuss both measles and scarlet fever there for the six year period 1929–1934 with respect to age and size of family on the basis of the records in the office of the Commissioner of Health of Providence which Dr. Smith kindly placed at our disposal. Before entering upon the detailed analysis by size of family we shall compare the incidence of the diseases with the figures of Dr. Chapin and of Dr. Pope. The comparisons cannot be made on a precisely equivalent basis because it seemed desirable to give definitions somewhat different from those which Dr. Chapin used. Thus he distinguished primary and secondary cases within the family, meaning by the latter any case arising during the same outbreak subsequent to the first without regard to whether it was presumably derived by infection from it, and when two cases arose on the same day he took one of them as primary and the other as secondary and did not specify in what way the selection was made.

[1] C. V. Chapin, "Measles in Providence, R. I., 1858–1923," *Am. Jour. Hygiene*, **5**, 635–655, 1925.

[2] A. S. Pope, "Studies on the Epidemiology of Scarlet Fever," *Am. Jour. Hyg.*, **6**, 389–430, 1926.

Indeed his discussion of his data ([1], p. 651) has led us to believe that we had better distinguish cases into primary and secondary for statistical purposes on the basis that all cases arising within the family within 6 days, *i.e.*, on the same day or on the next 6 days, are co-primary arising from infection outside the family, and only those appearing 7 days after the first case or cases as secondary within the family.[3] It is clear that these other children whose cases we have treated as secondary may have got their infection outside the family, and if one left an indefinite period between cases and subsequent ones, it would surely be generally true that the long delayed subsequent case was a renewed entry of measles into the family; we therefore had to use our judgment as to such re-entry and considered that a case occurring 26 or more days after the last previous should be treated as a new primary [4, 5]—in any event such instances are not at all common.

Comparison with Chapin's Series for Measles

2. *Attack and Secondary Attack Rates.*—We can, however, ignoring our own definitions, make up a table on the same basis as Chapin's Table 2 with the primaries when more than one occurs on the same day of entry into the family taken alternately as the youngest and the oldest child. (Had the youngest child of these simultaneous primaries been considered always as the primary the number of primaries at earlier ages would have been increased and at later ages decreased, while the number of secondaries at earlier ages would have been decreased and at later ages increased. This would not

[3] Percy Stocks and Mary N. Karn, "A Study of the Epidemiology of Measles," *Ann. Eugenics*, 3, 361–398, 1928, on p. 391 gives a similar estimate of 6 or 7 days. This reference contains a detailed analysis of the St. Pancras epidemic of 1926. We shall have several occasions on which to cite it.

[4] The figure of 26 days was derived by tallying and studying a large number of the individual family cards. Like the 6 or 7 days for the minimum interval between primary and secondary it is a statistical inference relative to the most practicable definition and does not mean that all prior to 26 days were actually true secondaries and all subsequent to 26 days were actually re-entries of measles into the family. Stocks and Karn [3] seem to use 20 days.

[5] Major Greenwood, "On the Statistical Measure of Infectiousness," *Jour. Hyg.*, 31, 336–351, 1931, to which we shall have other occasions to refer, seems to take one month (p. 342) instead of 26 days or 20 days.

change the attack rates but would alter somewhat the secondary attack rates by age.) It will be seen on comparison with Chapin's table (with adults omitted) that our attack rates and secondary attack rates are higher than his on the totals; but this is not true at every age for our attack rates are lower than his from 0 to 4 years of age inclusive, higher

TABLE 1

MEASLES. ATTACK RATE AMONG SUSCEPTIBLES, 1929–1934 WITH SECONDARY ATTACK RATES (CF. CHAPIN'S TABLE 2)

Age	Cases	Total Suscepti-bles*	Attack Rate	Primary Cases	Remaining Suscepti-bles*	Secondary Cases	Secondary Attack Rate
<1	324	658	49.2	96	562	228	40.6
1	671	752	89.2	179	573	492	85.9
2	849	922	92.1	270	652	579	88.8
3	963	1,042	92.4	326	716	637	89.0
4	1,102	1,182	93.2	416	766	686	89.6
5	1,340	1,405	95.4	735	670	605	90.3
6	1,519	1,569	96.8	1,130	439	389	88.6
7	1,339	1,383	96.8	1,051	332	288	86.7
8	988	1,033	95.6	754	279	234	83.9
9	517	573	90.2	359	214	158	73.8
10	265	298	88.9	167	131	98	74.8
11	156	195	80.0	91	104	65	62.5
12	54	80	67.5	27	53	27	51
13	33	56	59	23	33	10	30
14	30	52	58	22	30	8	27
15	15	29	52	10	19	5	26
16	13	20	65	9	11	4	36
17	2	10	20	2	8	0	0
18	4	12	33	4	8	0	0
19	8	17	47	5	12	3	25
20	8	18	44	6	12	2	17
Total	10,200	11,306	90.2	5,682	5,624	4,518	80.3
Chapin	14,190	15,952	89.0	7,904	8,048	6,286	78.1

* Include those who have the disease even though said to have had it before.

from 5 to 11, lower from 12 on, whereas our secondary attack rates are lower than his from 0 to 2, and higher from then on until the irregularities which occur after age 13 when the numbers have become very small in both his table and ours.

3. *Age Distribution of Cases.*—Table 2 is comparable with Chapin's Table 3. There is a notable difference between our

age distribution and his with ours lower from ages 0 to 4 inclusive, higher from 4 to 11 and lower from 12 on. The two distributions are illustrated in Fig. 1.

TABLE 2

MEASLES. 1929–1934. AGE DISTRIBUTION

Ages	Cases	Per Cent	Ages	Cases	Per Cent
<1	324	3.18	11	156	1.53
1	671	6.58	12	54	.53
2	849	8.32	13	33	.32
3	963	9.44	14	30	.29
4	1,102	10.80	15	15	.15
5	1,340	13.14	16	13	.13
6	1,519	14.89	17	2	.02
7	1,339	13.13	18	4	.04
8	988	9.69	19	8	.08
9	517	5.07	20	8	.08
10	265	2.60	Total	10,200	100.

4. *Rates of Incidence.*—The question arises: Did the incidence of measles upon the minor population really change between 1920 (Chapin's central year) and 1931–2 or was the incidence essentially the same and the age distribution of the child population quite different or did the reporting change? Table 3 gives the populations in 1920 and 1930, the average

TABLE 3

POPULATIONS AND AVERAGE CASES OF MEASLES, RATES PER THOUSAND AND RATIO OF RATES

Ages	1920 (1917–1923)			1930 (1929–1934)			Ratio
	Population	Cases	Rate	Population	Cases	Rate	
<1	4,913	114	23.2	3,809	54	14.2	0.61
1	4,775	197	41 3	3,926	112	28.5	0.69
2	4,804	221	46.0	4,168	142	34.1	0.74
3	4,532	240	53.0	4,411	160	36.3	0.68
4	4,425	238	53.8	4,357	184	42.2	0.78
5–9	22,186	918	41.4	23,745	950	40 0	0.97
10–14	20,432	80	3.92	22,961	90	3.92	1.00
15–19	18,434	17	0.9	22,586	7	0.3	0.30
Total	84,501	2,025	24.0	89,963	1,699	18.9	0 788

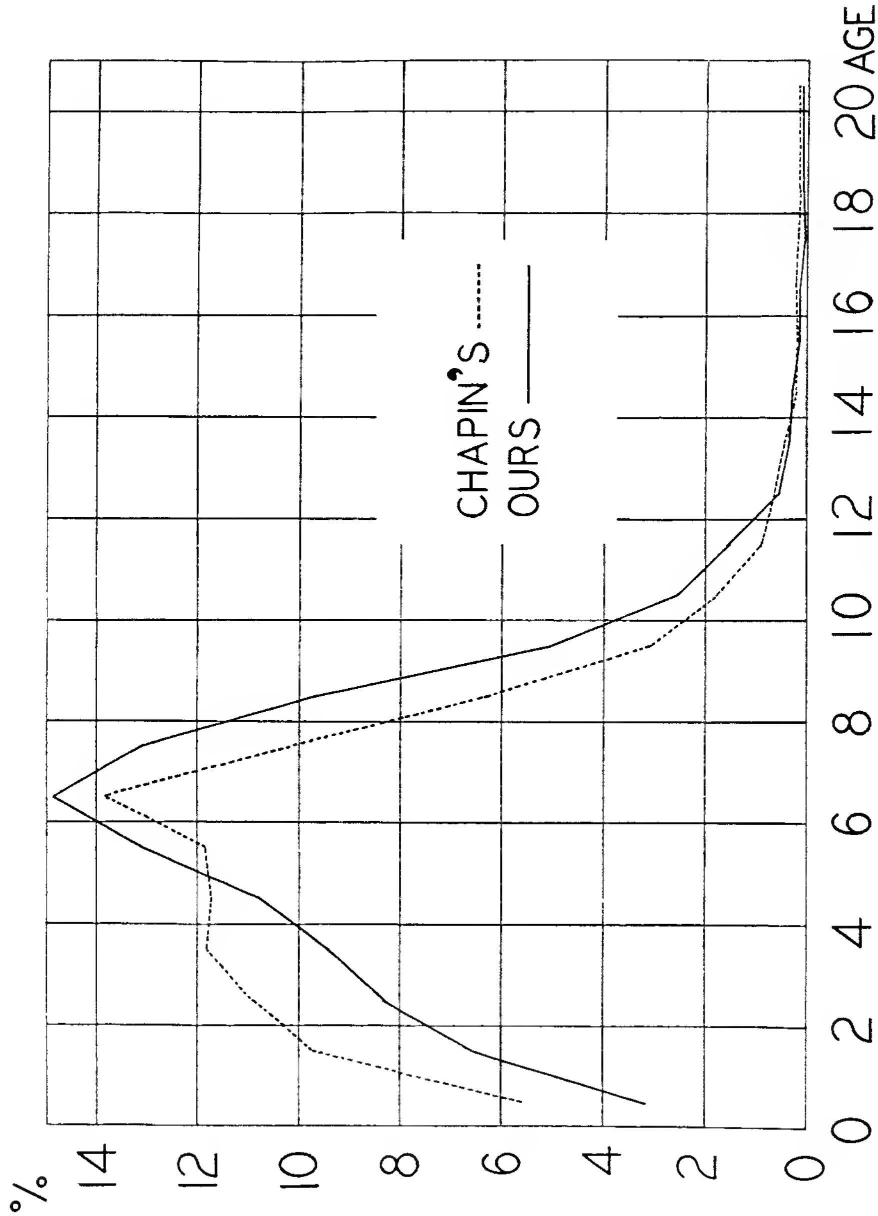

FIG. 1. Measles—percentage of total cases at each age.

cases of measles per annum for the two periods, and the rate per thousand and the ratio of the rates. Under five years there was less measles in the later period than in the former, whereas from 5 to 14 there was much the same amount, and after 15 there was less though the numbers here are very small. These results confirm in a general way the findings on the percentage distributions (Table 1). Indeed this form of presentation is so much to be preferred to a comparison of percentage distributions that the latter would not have been given had it been possible to find the minor population of Providence for 1920 and 1930 distributed by the U. S. Census over individual years. It should be remarked that there were probably further changes in the age distribution of the population between 1930 and 1932 because of lowered birth rates and perhaps because of unusual migration incident to the depression; we have however no way to calculate these changes.[6] Whether the reporting had changed materially in the dozen years 1920–1932 may be doubted; at any rate the fact that the rates are much the same would indicate that Chapin's remark that not half the cases which presumably arise are reported (assuming that most children ultimately have measles) remains practically unchanged, and while we believe that one of the effects of the depression was to cause persons to refrain from expenses for medical service we should doubt if the parents of pre-school children with measles would so conduct themselves in 1929–1934 compared with 1917–1923 as to produce a 25 per cent reduction in the reporting of measles in the ages 0–4.

[6] The estimates for the eight age groups in Table 3 might be taken as 3700, 3750, 3900, 4020, 4270, 22840, 23280, 22730 at a guess. This would give for the rates 14.6, 29.9, 36.4, 39.8, 43.1, 41.6, 3.87, 0.3 and for the ratios 0.63, 0.72, 0.79, 0.75, 0.80, 1.00, 0.99, 0.3, which would still show that while the rates from 5 to 14 were the same, those under 5 were materially lower. As reported therefore we must infer that the incidence on the pre-school child was relatively less at the later than at the earlier period—about three-quarters as much. (With a disease as variable as measles from year to year it could hardly have been expected that the rates based on 6 years 1929–1934 and on 7 years 1917–1923 should have been so nearly alike as they were in the groups 5 to 14. It may be noted that in 1929–1934 there were three years of relatively high incidence, whereas in 1917–1923 there were five.)

5. *Cases, Immunes and Susceptibles.*—Table 4 is like Chapin's Table 4 except further broken down. Immunes were those persons of whom it was stated that they had had measles; they are divided into those attacked and those who escaped—which makes possible a comparison with Chapin's Table 11. Susceptibles are those said not to have had the disease; some of these may have had it and furthermore many of those under one year of age (especially those under 6 months) would in reality surely be immunes. However, as we understand Chapin's definitions, his "cases" (column 2) correspond to our "total attacked," his "immunes" are our "immunes escaped" (which is the definition of immune which we use except in this table and except for a subsequent statistical correction for children under 1), his "non-immunes" are our "susceptibles escaped," and the final columns giving totals correspond. The division by sex, which Chapin does not give, had to be based upon the name of the child, which is not a safe criterion, especially with a large foreign population, and consequently we shall not emphasize sex differences.

In regard to comparisons between these tables, we have already seen that the age distributions of the cases are different. It is clear that the age distributions of the immunes, non-immunes (susceptibles escaped) and total children studied are also different. These differences apparently persist when the figures are referred to the child populations as was done for the cases. Indeed, as the immunes and non-immunes are respectively those children in families into which measles comes which have had measles before (except for the few who get it again) and those who remain even after the epidemic is past without having contracted the disease, the distribution by age of these groups might be expected, on a number of grounds, to differ if the age distribution of the cases differed. If C = cases, S = susceptibles, I = immunes, E = susceptibles escaped (non-immunes) and T = total children, the attack rate is $A = C/S$, $S = C + E$, and $T = C + E + I = S + I$. Hence $1/A = 1 + E/C$ or $E/C = 1/A - 1$, where A is measured as a fraction instead of as a percentage. If

TABLE 4

MEASLES. PROVIDENCE, R. I., 1929–1934. TOTAL CHILDREN STUDIED. ALLEGED IMMUNES AND SUSCEPTIBLES

Age	Immunes									Susceptibles									Total								
	Attacked			Escaped			Total			Attacked			Escaped			Total			Attacked			Escaped			Total Children Studied		
	M	F	T	M	F	T	M	F	T	M	F	T	M	F	T	M	F	T	M	F	T	M	F	T	M	F	T
<1		1	1	1		1	1	1	2	178	145	323	168	166	334	346	311	657	178	146	324	169	166	335	347	312	659
1				3	3	6	3	3	6	340	331	671	43	38	81	383	369	752	340	331	671	46	41	87	386	372	758
2	1	3	4	4	8	12	5	11	16	439	406	845	29	44	73	468	450	918	440	409	849	33	52	85	473	461	934
3	3		3	12	15	27	15	15	30	487	473	960	42	37	79	529	510	1,039	490	473	963	54	52	106	544	525	1,069
4	7	5	12	28	29	57	35	34	69	537	553	1,090	45	35	80	582	588	1,170	544	558	1,102	73	64	137	617	622	1,239
5	10	6	16	43	58	101	53	64	117	657	667	1,324	37	28	65	694	695	1,389	667	673	1,340	80	86	166	747	759	1,506
6	7	8	15	70	72	142	77	80	157	780	721	1,501	21	26	50	801	750	1,551	787	732	1,519	94	98	192	881	830	1,711
7	11	7	18	113	124	237	124	131	255	676	645	1,321	23	21	44	699	666	1,365	687	652	1,339	136	145	281	823	797	1,620
8	6	13	19	150	149	299	156	162	318	490	479	969	20	25	45	510	504	1,014	496	492	988	170	174	344	666	666	1,332
9	10	8	18	199	182	381	209	190	399	220	279	499	31	22	56	251	301	555	230	287	517	233	204	437	463	491	954
10	1	9	10	281	233	517	285	242	527	131	124	255	19	11	33	150	138	288	132	133	265	303	247	550	435	380	815
11	1	3	4	249	218	467	250	221	471	79	73	152	19	20	39	98	93	191	80	76	156	268	238	506	348	314	662
12	2		2	273	257	530	275	257	532	22	30	52	15	11	26	37	41	78	24	30	54	288	268	556	312	298	610
13		2	2	231	237	468	231	239	470	17	14	31	13	10	23	30	24	54	17	16	33	244	247	491	261	263	524
14	1	1	2	213	232	445	214	233	447	9	19	28	13	9	22	22	28	50	10	20	30	226	241	467	236	261	497
15				201	210	411	201	210	411	6	9	15	5	9	14	11	18	29	6	9	15	206	219	455	212	258	470
16	1		1	197	212	409	198	212	410	8	4	12	2	5	7	10	9	19	9	4	13	199	217	416	208	221	429
17				185	186	371	185	186	371	2		2	6	2	8	8	2	10	2		2	191	188	379	193	188	381
18				139	160	299	139	160	299	2	2	4	3	5	8	5	7	12	2	2	4	142	165	307	144	167	311
19	1	1	2	132	142	274	133	143	276	2	4	6	3	6	9	5	10	15	3	5	8	135	148	283	138	153	291
20		2	2	101	133	234	101	135	236	4	2	6	2	8	10	6	10	16	4	4	8	103	141	244	107	145	252
21				89	116	235	89	116	235		2	2	2	2	4	2	4	6		2	2	91	118	239	91	150	241
Total	62	69	131	2,917	3,036	5,953	2,979	3,105	6,084	5,086	4,985	10,071	567	543	1,110	5,653	5,528	11,181	5,148	5,054	10,202	3,484	3,579	7,063	8,632	8,633	17,265

therefore the attack rate differs in the two series, as we have seen, the ratio of remaining susceptibles to the cases differs, and differs inversely to the attack rate.

The previous discussion furnishes a comparison of cases and non-immunes. A comparison on totals, irrespective of age, may be made by considering the contingency table (Table 5):

TABLE 5

MEASLES. CASES, IMMUNES, NON-IMMUNES, 1917–1923 AND 1929–1934

Period	Cases	Immunes	Non-immunes	Total
1917–1923. . .	14,190 (58.7%)	8,206 (34.0%)	1,762 (7.3%)	24,158
1929–1934.	10,200 (59.9%)	5,718 (33.6%)	1,106 (6.5%)	17,024
Total	24,390	13,924	2,868	41,182

It appears that the fraction of non-immunes was greater in the earlier series. A more detailed consideration of the relation of cases to immunes shows that there is no significant difference, though the relations of cases to non-immunes and of immunes to non-immunes are both significantly different. The question however arises immediately as to whether these relationships upon totals are really of significance or whether they arise by accident of mixed classification. To settle this matter we must discuss the relationships in the detailed age groups. Let us take for example age 5 and make a table similar to the last:

1917–1923	1684 (80.2%)	318 (15.2%)	97 (4.6%)	2099
1929–1934.	1340 (89.0%)	101 (6.7%)	65 (4.3%)	1506

Here it is apparent, as must have been expected, that the distribution of the total children over the three categories is different, more being cases and fewer being immunes or non-immunes; but it is also apparent that between the cases and the immunes there is a significant difference in the two series, there being many more immunes relative to cases in the earlier series than in the later. A similar result holds for age 10. The statistical comparison of Chapin's Table 4 and our Table 4 could be pushed to great lengths, but we have been

unable to evolve therefrom anything epidemiological of much seeming significance.

6. *Recurrence in the Same Individual.*—We find, however, something of interest when we compare our Table 4 with Chapin's Table 11, giving second and subsequent attacks in the same person. The comparison is given in Table 6, from which it appears that in the later series there is a marked reduction in second and subsequent cases relative to cases and to immunes. This is probably as Chapin would hope; he seemed to feel strongly that there were actually fewer second

TABLE 6

MEASLES. 1917–1923 AND 1929–1934. SECOND AND SUBSEQUENT ATTACKS IN THE SAME PERSON AS RATIOS TO CASES AND TO IMMUNES

Ages	1917–1923		1929–1934	
	Per Cent of Cases	Per Cent of Immunes	Per Cent of Cases	Per Cent of Immunes
<1	0.2	28	0.3	100
1	0.8	58	0.0	0
2	1.2	28	0.5	33
3	1.5	19	0.3	11
4	2.7	19	1.1	21
5	3.9	21	1.2	16
6	5.3	25	1.0	11
7	6.5	16	1.3	8
8	10.1	13	1.9	6
9	10.5	7.0	3.5	5
10	13 1	4.9	3.8	2
11–13	21.9	3.0	3.3	0.5
14–20	23.4	1.7	8.8	0.3
Total	4.5	7.8	1.3	2.3

and subsequent attacks than were reported in his series ([1], p. 652). It is further clear that the incidence of the second and subsequent cases to cases or to immunes follows the same general manner of variation by age in the two series (though we do not mean to imply that a Chi-square test would show no significant difference).

7. *Days Between Primary and Later Cases.*—There remains one of Chapin's tables (No. 10) which gives the day, measured from the day of the first case in the family, on which later cases occurred. Now, we call the day of the first case "0"

and enter under it the number of cases in excess of one which occurred thereon; we use "1" for the day after the first. We are unable to determine what Chapin's definition was, but should judge from the figures that he may have combined

TABLE 7

MEASLES. 1917–1923 AND 1929–1934. DAY RELATIVE TO THAT OF THE FIRST CASE, RATED AS "0," ON WHICH SUBSEQUENT CASES OCCURRED IN THE FAMILY

Day	1917–1923		1929–1934	
	Cases	Per Cent	Cases	Per Cent
0	—	—	190	4.21
1	531	9.22	253	5.60
2	257	4.46	184	4.07
3	226	3.92	108	2.39
4	189	3.28	110	2.44
5	132	2.29	111	2.46
6	144	2.50	114	2.52
7	254	4.41	189	4.19
8	339	5.88	247	5.47
9*	283	4.91	388	8.59
10	401	6.96	519	11.49
11	500	8.68	511	11.32
12	416	7.22	451	9.99
13	463	8.04	344	7.62
14	467	8.10	244	5.40
15	381	6.61	142	3.14
16	188	3.26	105	2.33
17	99	1.72	58	1.28
18	69	1.20	37	0.82
19	48	0.83	25	0.55
20	47	0.82	26	0.58
21	55	0.95	23	0.51
22	47	0.82	13	0.29
23	23	0.40	19	0.42
24	19	0.33	14	0.31
25	14	0.24	13	0.29
26	13	0.23	15	0.33
27	9	0.16	10	0.22
28	15	0.26	3	0.07
29	12	0.21	6	0.13
30	18	0.31	2	0.04
30+	103	1.79	42	0.93
Total	5,762		4,516	

* The figure 283 for day "9" occurring between 339 and 401 on days "8" and "10" respectively seems peculiar; a similar observation may be made on the figure for day "12." Such irregularities make the application of a Chi-square test quite useless, because of their large contributions to the value of χ^2.

what we call "0" and "1" in his "1," for otherwise we are unable to explain the large figure which he gets for "1." The results in the two series are compared in Table 7. It is clear that the general sweep of the distributions is similar. We have an earlier mode or peak on the 9th, 10th, 11th, 12th days instead of the 11th, 12th, 13th, 14th days as found by Chapin and the mode is higher, which is more in accord with the definiteness of some clinicians' impressions as to the period between primary and secondary cases.[7] It was on the basis of this table and Chapin's discussion that we decided to take cases arising on days 0–6 inclusive as co-primary and to use 26 days as an elapsed period between cases after which the new case in the family would be called a new primary. A further critical analysis of these assumptions on a considerable amount of carefully controlled data would be desirable, but further analysis of this particular material would probably not pay.[8]

[7] Stocks and Karn [3] give a peak on 10th, 11th, 12th, 13th days, midway between ours and Chapin's.

[8] A. S. Pope in his *thesis* for the degree D.P.H. at Harvard gave an interesting discussion of this matter, which he did not include in his published paper.[2] Reference may also be made to W. L. Aycock and P. Eaton, "Comparison Between Multiple Cases of Measles, Scarlet Fever and Infantile Paralysis," *Am. Jour. Hyg.*, **5**, 733–741, 1925. We offer the following tabulation of the intervals between 1st and 2nd cases and between 2nd and 3rd cases for all families with just three cases. (If the three cases came down on the same day one entry was made as between 1st and 2nd cases and one as between 2nd and 3rd cases.)

Interval Between 1st and 2nd				Interval Between 2nd and 3rd			
Day	Cases	Day	Cases	Day	Cases	Day	Cases
0	68	11	72	0	252	11	19
1	61	12	66	1	146	12	17
2	43	13	53	2	89	13	16
3	21	14	39	3	52	14	9
4	25	15	14	4	36	15	1
5	20	16	8	5	18	16	7
6	20	17	2	6	27	17	2
7	40	18	0	7	22	18	4
8	41	19	2	8	20	19	1
9	75	20	2	9	23	20	2
10	110	20+	3	10	22	20+	0
Total			785	Total			785

8. *Males and Females.*—Returning to our Table 4, we may give the rates per thousand based on the whole period (not the annual rates) relative to the male and female population of Providence as given in the Census for 1930, as any changes to 1932 presumably affect both sexes sufficiently alike for purposes of comparison. In view of the numbers involved, which are by no means small except for a few entries, there is no important difference between the incidence of measles upon the sexes.

TABLE 8

MEASLES. RATES PER THOUSAND POPULATION BY SEX FOR TOTAL CHILDREN STUDIED, FOR CASES, FOR SUSCEPTIBLE ESCAPES (NON-IMMUNES), AND FOR IMMUNES

Ages	Male				Female			
	T C S	Cases	S E	I	T C S	Cases	S E	I
0–4	225	190	31.1	4.6	225	188	31.5	5.4
5–9	298	239	11.5	47.9	302	242	10.4	49 8
10–14	139	22.9	6.9	109	132	23.9	5.6	102
15–19	82.0	2 0	1.7	78.3	84.5	1.7	2.3	80.5
Total	188	115	12.5	60 7	185	112	11.8	61.2

9. *Comparison of Two Definitions of Primary and Secondary.*—We have stated that for our purposes we decided, somewhat regretfully, to depart from Chapin's definition of primary and secondary. The result is that our secondary attack rates as we should compute them should differ from those we gave in Table 1 for comparison with Chapin's Table 2. In Table 9 we give the comparison between Table 1 and the figures we get upon our definitions of primary and secondary. It is seen at once that with our definitions the secondary attack rates are lower, but should be more truly representative of the attack rate within the family.

10. *Immunity Under One Year of Age.*—A special study was made of the behavior of a sample of 623 children under one year of age by months when measles came into the family. It is clear that the total number of children involved (which

TABLE 9

MEASLES 1929–1934. PRIMARY CASES, OTHER SUSCEPTIBLES, SECONDARY CASES AND SECONDARY ATTACK RATES ON TWO DEFINITIONS

Ages	Chapin's Definitions				Our Modified Definitions			
	Primary	Other Susceptibles	Secondary	Secondary Attack	Primary	Other Susceptibles	Secondary	Secondary Attack
<1	96	562	228	40.6	138	520	186	35.8
1	179	573	492	85 9	262	490	409	83.5
2	270	652	579	88.8	377	545	472	86.6
3	326	716	637	89.0	465	577	498	86.3
4	416	766	686	89.6	547	635	555	87.4
5	735	670	605	90.3	876	529	464	87.7
6	1,130	439	389	88.6	1,260	309	259	83.8
7	1,051	332	288	86.7	1,163	220	176	80.0
8	754	279	234	83.9	822	211	166	78.7
9	359	214	158	73.8	412	161	105	65.2
10	167	131	98	74.8	189	109	76	69.7
11	91	104	65	62.5	105	90	51	56.7
12–14	72	116	45	38.8	84	104	33	31.7
15–21	37	75	15	20.0	41	71	11	15.5
Total	5,683	5,629	4,519	80.3	6,741	4,571	3,461	75.7

averages 52 in each month of life) is less than the average in the earlier and greater than the average in the later months. This may be due to some selective factor: indeed very few

TABLE 10

CHILDREN UNDER ONE YEAR OF AGE

Age (mos.)	Primary	Secondary	Escapes	Total	Secondary Attack
0–1	0	1	36	37	3%
1–2	0	0	38	38	0
2–3	2	2	36	40	5
3–4	3	0	46	49	0
4–5	9	9	44	62	17
5–6	5	6	29	40	17
6–7	17	19	34	70	36
7–8	17	21	16	54	57
8–9	18	28	10	56	74
9–10	21	33	11	65	75
10–11	20	20	7	47	74
11–12	19	39	7	65	85
Total	131	178	314	623	36

primary cases could come in during the first six months of life due to immunity from the mother. If the primary cases be deducted, the number of children under one year of age in each month of life in families into which measles comes averages 41 and the variation of the numbers about their mean is only very slightly in excess of the expected chance variation, viz., 7.6 ± 1.6 as against 6.4, and the total number in the first six months is the same as in the last six months of the first year.

The secondary attack rate under one year is 36 per cent as against figures of 86.6 per cent for the second year and 86.3 per cent for the third year (Table 9). It is clear that the secondary attack rate does increase from nearly nothing in the first four months of life through intermediate values to about the same values during the last months of the first year as are found for the immediately subsequent years. The ratio of 36 which is the average secondary attack rate during the first year to 86 which is the average for the second and third years of life is almost exactly as 5 : 12; thus statistically we may say, so far as this relatively small sample justifies an inference, that five-twelfths of the children under 1 may be considered to possess the same reaction to measles in the family as their immediately older siblings whereas seven-twelfths of them are behaving as immunes. Roughly and statistically speaking one is therefore justified in classifying children under 7 months of age as immunes and those from 7 to 12 months of age as susceptibles in a similar sense as these terms are used for children over one year of age.

Comparison With Pope's Series for Scarlet Fever and With Our Series for Measles

11. *Cases, Immunes and Susceptibles.*—The comparison of our series for scarlet fever with Pope's [2] will be briefer. We have fewer cases than for measles and he was interested in a variety of problems which do not here concern us. In the first place it should be remarked that the interval between primary cases and subsequent cases does not show the same

phenomenon which enables one to give a fairly definite period within which the cases may be called co-primary.[8] Measles passes mainly from child to child whereas scarlet fever is a carrier disease and many cases may arise from persons of any age who are themselves well. We therefore adopt the definition of primary cases as the first in the family and of subsequent ones as secondary cases.[9] Immunes could presumably be distinguished from susceptibles by a test as in diphtheria, with a considerable degree of unreliability, but as this method of discrimination was not applied in Providence (and usually is not applied in public health work) we have to fall back on the definition of an immune as one who is said to have had the disease and of a susceptible as one who is said never to have had it, though for the older children such a definition is patently very far from satisfactory. Table 11 is made up on the same basis as Table 4 for measles where immunes are divided into "attacked" and "escaped" like the susceptibles although for subsequent analysis we include the (few) attacked immunes among the susceptibles.

12. *Recurrence in the Same Individual.*—Pope found about one per cent of all cases to be recurrences in the same individual. Our Table 11 shows 61 recurrences (immunes attacked) and 3069 total cases or a ratio of two per cent. This variation is in the opposite direction to that observed in measles where our later series gave only about one-third as large a ratio of recurrences to cases as Chapin's earlier series. In our series for scarlet fever the ratio for males is $2.43 \pm .40$ per cent and for females $1.55 \pm .31$ per cent, the number of cases in males and females being essentially the same whereas the recurrences in males are half as much again as in females, but the numbers are so small that the difference is not quite significant. In Pope's earlier series the incidence of recurrences upon females was 163 as against 108 for males; much of the difference was among adults, the figures for minors being 90 males and 121 females; but total cases among minors

[9] There were in fact only 17 families out of 2417 which had two or more primary cases on the same day, all of which were simply counted as primary cases making a total of 2439 primaries.

TABLE II

Scarlet Fever. Providence, R. I., 1929-1934. Total Children Studied. Alleged Immunes and Susceptibles

Age	Immunes									Susceptibles									Total								
	Attacked			Escaped			Total			Attacked			Escaped			Total			Attacked			Escaped			Total Children Studied		
	M	F	T	M	F	T	M	F	T	M	F	T	M	F	T	M	F	T	M	F	T	M	F	T	M	F	T
<1										8	10	18	105	124	229	113	134	247	8	10	18	105	124	229	113	134	247
1										15	37	82	97	89	186	112	126	268	15	37	82	97	89	186	142	126	268
2				1		1	1		1	94	68	162	121	120	241	215	188	403	94	68	162	122	120	242	216	188	404
3	2		2	7	4	11	9	4	13	102	113	215	113	99	212	215	212	427	104	113	217	120	103	223	224	216	440
4	1	1	2	7	6	13	8	7	15	127	126	253	132	118	250	259	244	503	128	127	255	139	124	263	267	251	518
5	4	2	6	13	10	23	17	12	29	171	159	330	115	103	218	286	262	548	175	161	336	128	113	241	303	274	577
6	4	2	6	10	12	22	14	14	28	211	210	421	123	109	232	337	319	656	218	212	430	133	121	254	351	333	684
7	4	8	12	11	19	30	15	27	42	169	195	364	130	119	249	299	314	613	173	203	376	141	138	279	314	341	655
8	2	4	6	23	16	39	25	20	45	125	148	273	110	118	228	235	266	501	127	152	279	133	134	267	260	286	546
9	7		7	15	11	29	22	14	36	103	129	232	106	123	229	209	252	461	110	129	239	121	137	258	231	266	497
10	3		3	17	22	39	20	22	42	79	87	166	108	114	222	187	201	388	82	87	169	125	136	261	207	223	430
11		2	2	11	9	20	11	11	22	59	61	120	93	116	209	152	177	329	59	63	122	104	125	229	163	188	351
12	4	1	5	14	14	28	18	15	33	52	59	111	104	103	207	156	162	318	56	60	116	118	117	235	174	177	351
13	2	3	5	9	17	26	11	20	31	38	37	75	94	79	173	132	116	248	40	40	80	103	96	199	143	136	279
14	1		1	15	11	26	16	11	27	36	14	50	95	68	163	131	82	213	37	14	51	110	79	189	147	93	240
15	3		3	9	8	17	12	8	20	22	17	39	66	80	146	88	97	185	25	17	42	75	88	163	100	105	205
16		1	1	7	4	11	7	5	12	8	18	26	53	76	129	61	94	155	8	19	27	60	80	140	68	99	167
17				9	14	23	9	14	23	10	5	15	65	65	130	75	70	145	10	5	15	74	79	153	84	84	168
18				11	12	23	11	12	23	4	5	9	54	78	132	58	83	141	4	5	9	65	90	155	69	95	164
19				7	7	14	7	7	14	5	10	15	53	56	109	58	66	124	5	10	15	60	63	123	65	73	138
20				6	11	17	6	11	17	7	7	14	41	50	91	51	57	108	7	7	14	50	61	111	57	68	125
21				5	11	16	5	11	16	5	10	15	31	39	73	39	49	88	5	10	15	39	50	89	44	60	104
Total	37	24	61	207	221	428	244	245	489	1,483	1,525	3,008	2,015	2,046	4,061	3,498	3,571	7,069	1,520	1,549	3,069	2,222	2,267	4,489	3,742	3,816	7,558

are not separated by sex—still it appears that statistically our results and his with respect to sex incidence of recurrences are probably not to be reconciled. We may note that for measles we had no sex difference in the incidence of the recurrences (Table 4).

Dropping the distribution between the sexes and taking age distribution we have for scarlet fever and measles, both 1929–1934, the following figures:

Ages	Scarlet Fever			Measles		
	Recurrences	Per Cent Cases*	Per Cent Immunes†	Recurrences	Per Cent Cases*	Per Cent Immunes†
0–4	4	0.5	16	20	0.51	19.4
5–9	37	2.2	26	86	1.51	7.4
10–14	16	3.0	12	20	3.7	0.82
15–21	4	2.9	3	5	9.6	0.2
Totals	61	1.99	14.3	131	1.28	2.20

* Including the recurrences. † Excluding the recurrences.

The high percentages relative to immunes under age 5 are noteworthy and somewhat difficult to believe; the high percentages at older ages for scarlet fever as compared to measles are due, of course, to the small number of immunes ("who have had a case") compared with the real immunes who have acquired their immunity without a clinical case.

13. *Males and Females.*—Returning for a moment to the division between the sexes we may construct for scarlet fever a table analogous to Table 8 for measles giving rates per thousand for the six year period. There is no noteworthy difference between the sexes relative to the population. The incidence on females is greater (34.0) than on males (33.6) whereas the opposite was true for measles (112,115) and, though this may be taken as corroborative evidence provided such an inverse relationship is suspected from other sources, it is by no means significant in itself. It is probably natural to refer cases and total children studied and even immunes to the general population; less natural to do this with respect to

TABLE 12

Scarlet Fever. Rates per Thousand Population by Sex for Total Children Studied, for Cases, for Susceptible Escapes, and for Immunes

Ages	Male				Female			
	T C S	Cases	S E	I.	T. C. S.	Cases	S E	I
0–4	91.6	36.1	54.1	1.4	90.0	34.9	54.1	1.0
5–9	121.6	66.9	48.7	6.0	127.7	73.0	48.7	6.0
10–14	72.7	23.9	43.0	5.8	71.1	23.0	41.8	6.4
15–19	35.4	4.8	26.7	3.9	39.1	4.8	30.4	3.9
Totals	81.1	33.6	43.1	4.4	81.8	34.0	43.4	4.4

susceptible escapes which should presumably be referred to total susceptibles as is done with the secondary attack rate.

14. *Rates of Incidence.*—We take Pope's 20-year series 1904–1923 with the populations of 1910 and 1920 averaged, to compare with our 6-year series. Of course with diseases as variable in their morbidity as measles and scarlet fever, rates based on a short period such as six years are probably not worth much, except as variation by age might be similar even when total amounts were not. We give below our rate per thousand per annum for measles, the cases and rate for scarlet fever, the ratio of measles to scarlet fever, and the rate for scarlet fever (which we have computed as indicated from Pope's data) in broad age groups:

Age	0–4	5–9	10–14	15–19	Total
Meas. Rate.	31.5	40.0	3.92	0.3	18.9
Sc. F. Cases	122	277	90	18	507
Sc. F. Rate.	5.90	11.7	3.92	0.8	5.64
Meas./Sc. F..	5.34	3.42	1.0	0.4	3.35
Pope's Sc. F. Rate	4.38	7.52	2.89	0.8	3.98

Pope showed that morbidity from scarlet fever had remained practically constant since 1885; in view of the great variations in morbidity from year to year, the fact that our six-year series shows a rate 40 per cent higher than Pope's need not mean that morbidity has increased.[10]

[10] The Chapin and Pope series and ours are of cases at home. Moreover they are of reported cases. If reporting improves, morbidity will appear to increase

Chapin (Table 8) gives for 1917–1923 a distribution of scarlet fever by ages on a comparable basis with measles. We list in Table 13 the cases for comparison with our series of each.

TABLE 13

COMPARISON OF CHAPIN'S FIGURES 1917–1923 FOR CASES OF MEASLES AND SCARLET FEVER WITH OURS (1929–1934) WITH THE RATIOS MEASLES TO SCARLET FEVER AND THE RATIO OF THE TWO RATIOS

Age	1917–1923			1929–1934			Double Ratio
	Measles	S. F.	Ratio	Measles	S. F.	Ratio	
<1	797	17	47	324	18	18	2.60
1	1,381	73	18.9	671	82	8.20	2.30
2–4	4,890	627	7.80	2,914	634	4.60	1.70
5–9	6,425	1,150	5.59	5,703	1,660	3.44	1.62
10–14	560	455	1.23	538	538	1.00	1.23
15–19	121	146	0.83	42	108	.39	2.1
Total	14,174	2,468	5.74	10,192	3,040	3.35	1.71

From the comparison it appears very clear that there happened to be more measles in 1917–1923 relative to scarlet fever than in 1929–1934; and, further, that the ratios are following a different variation with age, for the totals show a ratio of ratios as of 1.71 whereas the individual age groups show ratios varying from 1.23 to 2.6. We have seen in discussing measles that apparently the change was partly due to the change in the population distribution, but not wholly, because there seems really to be relatively a lower incidence later than there was earlier at the younger ages. We may make a comparison similar to that of Table 3 for average annual rates per thousand population for scarlet fever in the two periods and the ratio of the rates:

Age	<1	1	2–4	5–9	10–14	15–19	Total
Chapin's . . .	0.4	2.1	6.54	7.39	3.18	1.14	4.17
Ours	.8	3.6	8.19	11.67	3.92	0.80	5.64
Ratio	2.0	1.71	1.25	1.58	1.23	0.70	1.35

Our rates are uniformly higher except in the last age period but the ratios are far from constant showing a different age distribution of the incidence.

15. *Age Distribution of Cases.*—Because percentage age distributions are often given and used for comparison, we append a table of them based on Pope's series 1887–1920 and ours of 1929–1934 by single years of age, and on Chapin's of 1917–1923 and ours based on the age intervals he used. On a percentage basis as compared with Chapin, we have reductions in all groups but that (5–9) which contains the peak whereas on a rate basis the first two groups show relatively a greater intensification than the 5–9 group. As compared with Pope's longer and earlier series we have lower percentages through age 4, higher from 5 to 13 and lower from 14 on. As compared with the older series of Pope both Chapin and we show smaller values in the earlier years and higher values at the peak, with Chapin's values intermediate between Pope's and ours; but in the later years no such simple statement describes the relation between the three series.

TABLE 14

Scarlet Fever. Percentage Age Distributions of Cases

Ages	1887–1920		1917–1923	1929–1934	
<1	2.0		0.7	0.6	
1	4.4		3.0	2.7	
2	7.7			5.3	
3	9.7	27.4	25.4	7.1	20.9
4	9.8			8.3	
5	10.5			11 0	
6	10.6			14.1	
7	9.8	45.2	46.6	12.3	54.6
8	7.8			9.1	
9	6.2			7.8	
10	5.0			5.5	
11	3.7			4.0	
12	3.0	16.1	18.4	3.8	17.7
13	2.3			2.6	
14	2.0			1.7	
15	1.3			1.4	
16	1.1			0.88	
17	1.0	4.9	5.9	0.49	3.6
18	0.75			0.29	
19	0.74			0.49	
20	0.64			0.46	

16. *Attack and Secondary Attack Rates.*—Table 15 gives the attack rates by age for two series of Pope's, 1887–1920,

1904–1923, the secondary attack rate for the latter and the primary and secondary cases with attack and secondary attack rates for our 1929–1934 series. It may be observed that Pope's later series, which overlaps his earlier one through a good many years, has decidedly lower attack rates by age: it

TABLE 15

Scarlet Fever. Attack Rates and Secondary Attack Rates

Age	1887–1920	1904–1923		1929–1934			
	Attack	Attack	Secondary Attack*	Cases		Attack	Secondary Attack
				Primary	Secondary		
<1	16.5	6.9	4.6	9	9	7.3	3.8
1	38.0	20.9	14 5	51	31	30.6	14.3
2	52.5	35.0	23.1	107	55	40.2	18.6
3	58.0	41.5	25.9	142	75	50.6	26.1
4	60 4	43.9	31.8	180	75	50.5	23.1
5	60.4	49.9	28.6	263	73	60.6	25.1
6	62.8	52.8	26.0	375	55	65.0	19.2
7	59 7	51.3	27.1	319	57	60.2	18.6
8	55.4	45 9	21.3	228	51	55 0	18.3
9	48.7	41 8	19.5	195	44	51.1	16.1
10	46.4	39.7	13.6	150	19	43.2	7.9
11	40.9	34.8	14.8	104	18	36.9	7.9
12	35.9	31.8	12 8	94	22	35.9	9 6
13	32 1	28.3	6.7	67	13	31.6	7.0
14	30 3	28.1	6.9	43	8	23.8	4.7
15	24.3	19.7	7.8	36	6	22.3	3.9
16	21.9	15.9	4.5	22	5	17.3	3.7
17	22 2	17.3	4.7	12	3	10 3	2.3
18	18.3	14 2	4.4	7	2	6.4	1.5
19	20 6	14.6	4 6	11	4	12.1	3.5
20	18.2	14.2	2.0	12	2	13.0	2.1
Total	46.1	—	—	2,427	627	43.4	13.6

* Pope gives these by sex: we have taken the mean of the two rates.

also runs lower than ours which indeed resembles his earlier series fairly closely except that our attack rates at ages under 5 seem definitely lower than his (though not so low as those of his later series). Our secondary attack rates are lower than his, though considering the small numbers involved little confidence is to be placed in any one item of our series or of

his. That attack rates and secondary attack rates for scarlet fever should be well below those for measles is clear.

Size of Families and Age Distribution of Children

17. *Families with Measles or Scarlet Fever.*—The number of families which had at least one case of measles in an individual 21 years of age or younger during the six years 1929–34 was 5665; for scarlet fever the number was 2417; these numbers are in the ratio 2.34 to 1 or about 7 to 3. The families counted may involve a certain number of repetitions; the records are kept on the basis of the first case occurring in the family during an outbreak of the disease and it is possible, indeed probable, that the disease entered the same family during different outbreaks. To study the history of the entrance of the disease into families for a long period would be of great interest but cannot well be undertaken by us on the records available. Stocks and Karn have some interesting observations from following families for a few years.[11]

Table 16 gives the number of families by size, *i.e.*, by the number of persons 21 years of age or younger in the family, all cases and contacts at older ages being ignored. The per-

TABLE 16

Number and Percentage of Families by Size for Measles and Scarlet Fever

Size	Measles	Per Cent	Scarlet Fever	Per Cent
1	1,086	19.2	350	14 5
2	1,581	27.9	698	28.9
3	1,214	21.4	591	24 5
4	765	13 5	362	15.0
5	430	7.6	176	7.3
6	269	4.7	109	4.5
7	147	2.6	54	2.2
8	89	1.6	38	1.6
9	43	0.76	21	0.87
10+	41	0.72	18	0.74
Totals	5,665	100	2,417	100

[11] See reference 3, pp. 376–383, with a suggestion for the explanation of the two year periodicity of measles See also *Lancet*, May 20, 1933, p. 1086.

sons in the families will be called siblings, as in most instances they were, although in some instances they included some who were not siblings. All families of ten or over are lumped together.

As the population for both diseases is that of Providence during the same six years and as these distributions are clearly different, one may infer that measles and scarlet fever behave differently with respect to the selection of families by size, measles selecting relatively more one-child families: indeed this is the chief difference, for if the one-child families be thrown out and the Chi-square test be applied to the rest of the distributions there remains no significant difference ($P = 0.5$). It will be noted that for both diseases the modal family is two. It is desirable to express these data not as percentage distributions but as rates.

18. *Rates of Incidence by Families.*—The U. S. Census gives the number of families in Providence by size when determined by the number of persons *under* 21. Although our figures contain persons 21 years of age, measles and scarlet fever affect so few persons 21 years of age that probably not much error will be made by using our classification and that of the Census simultaneously. The "rate per cent" in Table 17 is the percentage of the families of given size in Providence which had measles (or scarlet fever) come into them during

TABLE 17

DISTRIBUTION OF PROVIDENCE FAMILIES BY SIZE IN 1930 AND RATE PER HUNDRED OF INCIDENCE OF MEASLES AND SCARLET FEVER IN THE SIX YEARS 1929–1934 UPON THESE FAMILIES

	Providence	Per Cent	Measles	Rate Per Cent	Scarlet Fever	Rate Per Cent
1	11,511	32.9	1,086	9.4	350	3.0
2	9,245	26.4	1,581	17.1	698	7.6
3	6,067	17.3	1,214	20.0	591	9.7
4	3,590	10.2	765	21.3	362	10.1
5	2,105	6.0	430	20.4	176	8.4
6–8	2,256	6.4	505	22.4	201	8.9
9–	264	0.75	84	31.8	39	14.8
Total	35,038	100	5,665		2,417	

the six year period.[12] The figures are interesting; they would be even more interesting if we could follow the "rate per cent" down to the larger families without grouping. The table shows that the modal number of minors in a family in Providence is 1 and that the chances that measles gets into a family of 3 or more children are nearly equal; for scarlet fever the chances are nearly equal for the 3 and 4 child families.

If we call p the chance that a child bring measles into the family and q the chance that he does not and if we take $p = 0.094$, $q = 0.906$ for the one-child family, the chance that neither child (assumed to be acting independently) of a two-child family should bring measles in is $q^2 = 0.821$ and the rate of incidence on the 2-child family should be 0.179 whereas it is 0.171. Such a "theory" is clearly doubtful because the chances should depend on the ages of the children and there is little reason to suppose that an average chance could adequately replace the whole group of chances due to the different ages; moreover, there is no reason to suppose that the children act independently. As a matter of fact the theory clearly breaks down if we try to extend it to larger sized families, and it does not work for scarlet fever even for 1- and 2-child families. A thorough treatment of the epidemiology of an infectious disease must clearly discuss size of family because the family represents a closer contact than most other contacts as indicated by the secondary attack rates which are high compared with the chances of getting a disease in the community. Our study of size of family is offered as a beginning of such a treatment of epidemiology.

A census was taken in Boston[13] in January 1934. Sizes of families were not tabulated in the published reports but we

[12] The families into which one of the diseases enters may be double counted, as remarked before; also the definition of size of family may differ slightly. It must further be borne in mind that we can only enumerate reported cases of measles and of scarlet fever, and the fraction of reporting may be itself a function of size of family.

[13] This census was a "white-collar" project of the Civil Works Administration and Federal Emergency Relief Administration. Publication was under the title Report on the Census of Unemployment in Massachusetts.

took out a sample of about 8 per cent which we shall discuss below. The distribution of families by size was:

1	2,623	31.8%	(32.6)	6	286	3.46		
2	2,216	26.8	(26.8)	7	154	1.86	6.4	(6.0)
3	1,450	17.6	(17.4)	8	91	1.10		
4	887	10.7	(10.6)	9	26	0.31	0.47	(0.57)
5	515	6.2	(6.1)	10+	13	0.16		
					8,261			

This Boston distribution is remarkably similar to that of Providence. A comparison of the U. S. Census figures for Boston in 1930 is added in parenthesis. Of course, there may have been changes between 1930 and 1934 in the distribution in Boston but the sample is running pretty fair, and is probably good enough to justify us in breaking down the 6.4 per cent of the group 6–8 for Providence into 3.4_6, 1.8_6, 1.1_0 and thus estimate the number of families with 6, 7 and 8 children as 1216, 653, 387 and the "rate per cent" for the incidences of measles upon these families as 22.1, 22.5, 23.0 respectively; the results for scarlet fever are respectively 9.0, 8.3, 9.8. We cannot treat the 9+ group very well because the total fraction of such families in Providence appears to be greater than in Boston and it would therefore be quite doubtful to place two-thirds of them in the 9 and one-third of them in the 10+ groups. There does however seem to be some evidence that as the size of the family increases above 5 the "rate per cent" of incidence of either measles or scarlet fever tends slightly to increase; the numbers are too small and the calculations too hypothetical to say more.

19. *Age Distribution of Children in Families by Size.*—Whether the encouragement the figures give us to place some confidence in carrying over to Providence the results of analysis of our sample of the Boston 1934 census is justified or not, we can apparently get from no other source even a rough estimate of the important statistical basis for computing rates, viz., the age distribution of children by size of family. We shall therefore here present some comments on the Boston 1934 census and our sample of 8 per cent of it. General com-

ments are as follows: The 1934 figures for ages "under 1" and "1" are low compared with those of 1930; the birth rate was declining but the change seems excessive and suggests that there may have been under-enumeration in 1934 in these groups; our sample is still lower. Indeed our sample seems to run low to age 8 and high thereafter. Entirely apart from this systematic variation, the magnitude of the departures are such that, judged by the Chi-square test, the sample is unfair ($P = 0.02$). Despite these defects we have to use the sample for whatever indication we get with respect to age distribution of children in families by size of family.

TABLE 18

POPULATIONS AND PERCENTAGE DISTRIBUTION FOR AGES 0 TO 20 IN BOSTON 1930, IN BOSTON 1934, AND IN OUR SAMPLE OF 8 PER CENT THEREOF

Ages	Boston 1930		Boston 1934		Sample 1934	
	Population	Per Cent	Population	Per Cent	Population	Per Cent
<1	11,704	4.28	8,856	3.46	668	3.27
1	12,209	4.46	10,040	3.93	760	3.72
2	12,831	4.69	11,776	4.61	902	4.42
3	12,906	4.72	12,297	4.81	924	4.52
4	12,724	4.65	12,112	4.74	895	4.38
5	13,062	4.78	12,352	4.83	935	4.58
6	13,410	4.90	12,641	4.94	988	4.84
7	13,140	4.80	12,182	4.76	947	4.64
8	13,249	4.84	12,150	4.75	969	4.74
9	13,368	4.89	12,463	4.87	1,013	4.96
10	13,108	4.79	12,998	5.08	1,050	5.14
11	12,646	4.62	11,935	4.67	1,013	4.96
12	13,566	4.96	13,234	5.18	1,096	5.37
13	12,681	4.64	12,558	4.91	1,048	5.13
14	12,997	4.75	12,802	5.01	1,070	5.24
15	12,662	4.63	12,187	4.77	962	4.71
16	13,256	4.85	12,776	5.00	1,047	5.13
17	12,676	4.64	12,376	4.84	1,044	5.11
18	13,644	4.99	12,698	4.97	987	4.83
19	13,727	5.02	12,627	4.94	1,083	5.30
20	13,911	5.09	12,626	4.94	1,021	5.00
Total	273,477		255,686		20,422	

We took out the numbers and percentage distributions by age of children in the Boston sample for the ten different sized

families. It appeared that the distributions showed small differences for 4- and 5-child families and these were accordingly combined; the distributions for 6-, 7-, 8-, 9- and 10+ families seemed so similar that we preferred to combine them all to get greater statistical stability. The 1-, 2-, and 3-child families seemed different from one another and from the other two groups. In Table 19 we give the percentage distributions. That there are large fluctuations of the sort we should judge to be statistical are clear. For example, the course of the percentages in the 1-child family immediately before and after and at age 9 is so erratic as to indicate accident. A similar fluctuation appears at age 15. The numbers are small but such a set as 103, 77, 133 at ages 14, 15, 16 fluctuates too violently to be easily ascribed to pure chance. The prac-

TABLE 19

Boston Sample for 1934. Percentage Distributions by Age of Children in Families of Different Size and for all Children 21 Years of Age or Younger

Age	1	2	3	4–5	6+	Total
<1	5.9	3.5	2.9	2.3	2.3	3.1
1	6.0	4.3	3.4	2.6	2.6	3.5
2	5.4	5.1	4.4	3.4	3.5	4.2
3	4.5	5.1	5.2	3.5	3.7	4.3
4	3.2	4.3	4.8	4.0	4.3	4.2
5	3.3	4.5	4.4	4.6	4.5	4.4
6	3.3	4.3	4.9	4.8	5.3	4.6
7	2.4	3.9	4.6	5.1	5.1	4.4
8	1.9	3.7	4.9	5.3	5.6	4.5
9	3.0	4.0	4.3	5.5	5.9	4.7
10	2.4	4.8	4.2	5.8	6.2	4.9
11	2.2	4.1	4.6	5.7	5.8	4.7
12	2.8	4.3	5.2	5.9	6.1	5.1
13	3.2	4.3	4.5	5.6	6.0	4.9
14	3.9	4.1	4.9	5.7	5.7	5.0
15	2.9	4.1	4.7	5.1	4.8	4.5
16	5.1	4.9	4.9	5.0	4.5	4.9
17	5.0	4.8	5.1	4.7	4.9	4.9
18	5.9	4.8	4.6	4.1	4.3	4.6
19	7.3	6.0	4.7	4.5	3.7	5.1
20	9.6	5.5	4.6	3.3	3.2	4.8
21	10.8	5.5	4.1	3.6	3.0	4.7
12–14	9.9	12.7	14.6	17.2	17.8	15.0
15–21	46.6	35.6	32.7	30.3	28.4	33.5

tical statistician cannot avoid taking into consideration as accidental fluctuations some which may be larger than would arise by pure chance: he has to allow for fluctuations for which he cannot correct—*i.e.*, for his ignorance. It seems to be true that in all sizes of families, and on the total, the percentage of children at age 15 tends to be low; this may be a reality in that births fifteen years earlier may have been notably deficient, but it may be an artifact due to reporting to the enumerators in our 1934 Boston Census, or it may be partly real and partly artificial. We do not wish to take Table 19 too seriously, we are interested in Providence in 1932; all we need is to get some idea of the population base for rough rate making purposes sufficient to prevent us from comparing too carelessly distributions of measles and scarlet fever by size of family.

From Table 19 or from Fig. 2 one may see that total children run fairly constant except for the first five years of life, the smaller numbers in the earlier years being due partly to the lowering of the birth rate and partly in all probability to an under-enumeration. Contrary to this generally uniform course in totals, children in 1-child families exhibit a definitely U-type distribution with the minimum from 7 to 11 years of age less than half the value under two years and less than a quarter the value at 20 or 21. The course of the curve in its later portion is not particularly important in the study of childrens' diseases because cases at those ages are so few, but large variations in the earlier portion are important. The 2-child family shows a maximum around ages 2–3, a minimum from 7–9, rising to a maximum at 19–21. The 3-child family follows a distribution not very different from that of the total except for a tendency to a maximum at age 3. Families of 4–5 or 6+, however, follow a bell-shaped curve with a maximum from 10–13 approximately twice as high as the extremities. As our aim is to get rates we could draw up a table of Providence child populations by age and size of family if we should assume (1) that the distribution of children over families by size is the same in Providence as in

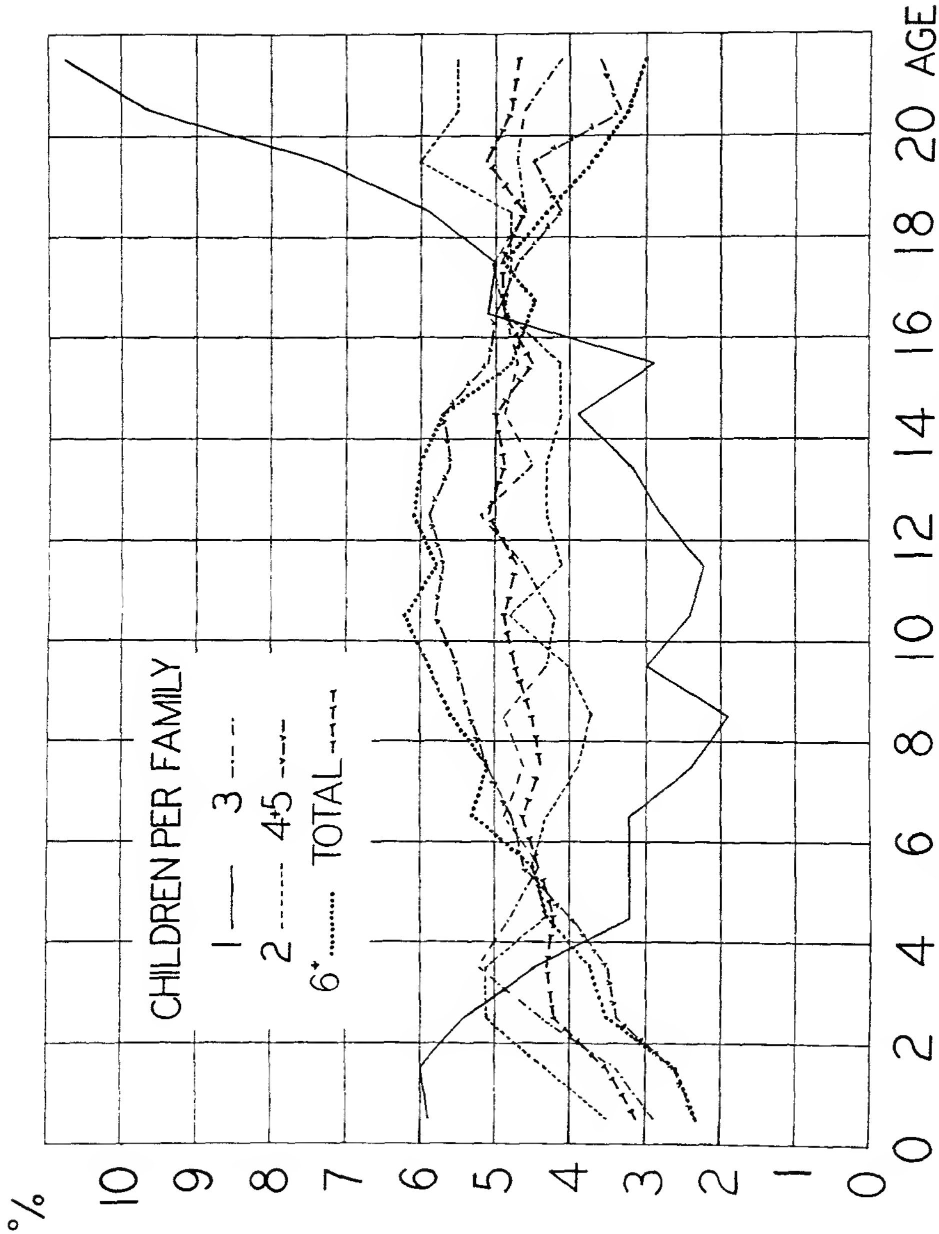

FIG. 2. Boston sample—percentage age distribution by size of family.

Boston and (2) that the distribution by age within the families of given size was as in Boston (Table 19). Such figures would not check too well with total Providence populations by age.

20. *Distribution of Children of Given Age Over Families.*—We could proceed the other way around and take the Providence population by age (which would necessitate distributing the 5-year age groups above 4 years of age) and draw from the Boston sample the percentage distribution of the children of a given age over sizes of family. As such a table would have some demographic interest we give it (Table 20). Indeed, if there be under-enumeration in some groups and if the tendency to under-enumeration is the same in families of all sizes it may be that it is better to apply Table 20 to popula-

TABLE 20

BOSTON SAMPLE. PERCENTAGE DISTRIBUTION OVER SIZE OF FAMILY OF CHILDREN OF DIFFERENT AGES

Age	1	2	3	4–5	6–
<1	23.2	23.5	19.2	20.7	13.5
1	20.8	25.1	19.7	21.1	13.3
2	15.7	25.1	21.1	23.1	15.1
3	12.7	24.2	24.4	22.9	15.8
4	9.3	21.2	23.5	27.2	18.9
5	9.2	21.2	20.6	30.1	18.9
6	8.7	19.1	21.4	30.0	20.9
7	6.7	18.1	21.3	33.1	20.9
8	5.2	17.1	21.8	33.5	22.4
9	7.8	17.7	18.6	33.3	22.7
10	6.0	20.2	17.2	33.6	23.0
11	5.6	18.2	19.8	34.2	22.3
12	6.8	17.5	20.6	33.2	21.9
13	7.9	18.1	18.8	32.8	22.3
14	9.6	16.8	20.1	32.6	20.8
15	8.0	19.0	21.2	32.2	19.5
16	12.7	20.8	20.3	29.4	16.7
17	12.6	20.4	21.3	27.4	18.3
18	15.7	21.7	20.2	25.6	16.8
19	17.7	24.6	18.8	25.4	13.5
20	24.6	24.0	19.7	19.7	12.0
21	28.1	24.3	17.7	21.9	7.9
Total	12.2	20.7	20.3	28.6	18.2

tions given by age than to use the two assumptions necessary with Table 19.

21. *Illustrating Rates by Size of Family and Age.*—We estimated that in 1932 the Providence populations in the first 5 years of life were [6] 3700, 3750, 3900, 4020, 4270. We are not very confident of the accuracy of these estimates, but they were made by carefully considering births (resident and non-resident), deaths, and the 1930 Census figures. We find then the following distribution over families:

Age	Size of Family				
	1	2	3	4–5	6+
<1	858	870	710	766	500
1	780	941	739	791	499
2	612	979	823	901	589
3	511	973	981	921	635
4	397	905	1,003	1,161	807

The following table gives the primary cases of measles in these 25 groups:

Age	1	2	3	4–5	6+
<1	20	32	30	36	20
1	47	75	55	52	33
2	65	103	80	89	40
3	61	137	113	97	57
4	68	157	141	119	62

These figures lead to the following table of rates per thousand for the incidence of primary cases of measles during the whole six year period 1929–1934:

Age	1	2	3	4–5	6+
<1	23	37	42	47	40
1	60	80	74	66	66
2	106	105	97	99	68
3	119	141	115	105	90
4	171	173	141	102	77

It is obvious the rates are increasing with age in the three smaller families but tend to become constant or even to decrease in the two larger groups, and also that the rates in a given family size are not by any means proportional to the cases. With the small numbers involved none of the rates are worth much, but as estimates of relative incidence within each family size they are probably better than the cases themselves (which is all one really gets by a percentage distribution of cases on age)—at any rate they are quite different. Having illustrated this point we shall, in view of the fact that few health officers have available populations by size of family, revert for the most part to percentage distributions, which all can make from their own records.

22. *The Structure of the Two-Child Family.*—Before leaving the discussion of the age distribution of the children in the Boston sample we give in Table 21 the frequency of pairs of ages in the 2-child families.[14] In most cases these pairs are

TABLE 21

BOSTON SAMPLE. FREQUENCY OF PAIRS OF AGES IN 2-CHILD FAMILIES

	<1	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	Total
<1	2	13	35	15	10	13	14	4	2	5	2		3		1	1	2	2	1	8	9	13	155
1			21	39	18	18	10	10	5	9	4	2			3	1	1	3	5	6	11	12	178
2			1	35	32	21	23	8	5	4	5	3		2	2	2	3	2	2	9	3	7	169
3					24	27	19	14	10	9	6	4	3	4	1		2	3	1		2	6	135
4					1	9	21	14	11	9	6	6	5	3	2	2	4	1		7	2	2	105
5							18	19	16	11	14	7	3	8	1	1	1	1	4	2	3	1	110
6							1	8	14	15	10	12	6	3		4	2	1	3	4			83
7								1	15	22	18	6	6	6	2	2	3	1	4	3	3	1	93
8									1	6	14	20	8	13	9	1	4	3	3	2		3	87
9										3	9	13	14	8	6	3	8	7	7	4	3	1	86
10											1	12	30	20	18	7	5	8	4	3	9	6	123
11												1	9	26	9	7	9	10	4	5	10	7	97
12														6	17	13	15	14	9	12	11	8	105
13															5	24	12	12	10	12	5	11	91
14															1	8	25	15	8	14	21	11	103
15																1	13	24	16	21	19	12	106
16																		7	32	25	27	18	109
17																		4	14	40	21	16	95
18																			2	18	34	31	85
19																				2	23	44	69
20																					4	21	25
21																						7	7
Total	2	13	57	89	85	88	106	78	79	93	89	86	87	99	77	77	109	118	129	197	220	238	2,216

[14] To avoid the awkward circumlocutions necessary for precise statement we speak of the 2-child family, meaning a family with two persons 21 years of age or younger (and with possibly other persons of greater age) living together. In a pair of ages such as under 1 and 21 it is entirely possible that the older of the two "children" is a parent (presumably, mother).

siblings but not in all; we cannot, for example, assume that the 33 pairs of equal ages are twins. With the large number of possible pairs of ages (253) the numbers can average only 8.8 in the cells and it is not easy to see any very definite mode of variation. The numbers and percentages at a definite number of years apart in age are:

Years	0	1	2	3	4	5	6	7	8	9	10
Numbers . .	33	294	532	332	243	192	144	82	77	57	37
Per Cent .	1.5	13.3	24.0	15.0	11.0	8.7	6.5	3.7	3.5	2.6	1.7
Years	11	12	13	14	15	16	17	18	19	20	21
Numbers	22	17	22	11	15	10	20	16	26	21	13
Per Cent	1.0	0.8	1 0	0.5	0.7	0.5	0.9	0.7	1.2	0.9	0.6

To give the structure of the families of more than 2 children by ages would require multiple correlation tables, and with the large number of cells and the small numbers in each, it would be futile—although a matter of great interest if only the sample were large enough to make a real study of the structure. We shall merely list in Table 22 the age distributions of the children by birth orders, as oldest 1, next 2, etc., and by age in the smaller families with the total numbers of children involved, repeating for comparison the percentages for the one-child family.

Measles by Size of Family and by Age

23. *Percentage Age Distribution.*—In making Table 23 of total cases of measles by age and size of family the ages 12–14 inclusive and 15–21 inclusive have been combined because of the small numbers. The last line gives the cases per family and rises steadily from 1 for the 1-child family to 3.39 for families of 10 and up. Some differences in the distributions by age appear at once. The fractions of cases in the lower age groups tend to rise with size of family and the fractions in the higher age groups tend to drop. In all but the largest

TABLE 22

THE BOSTON SAMPLE. PERCENTAGE DISTRIBUTION BY AGE OF CHILDREN BY SIZE OF FAMILY AND BY BIRTH ORDER

Family	1	2 1	2 2	3 1	3 2	3 3	4 1	4 2	4 3	4 4
<1	5.9	0.1	7.0	—	0.1	8.8	—	—	0.2	9.7
1	6.0	0.6	8.0	—	1.0	9.4	—	—	1.2	10.0
2	5.4	2.6	7.6	0.1	4.3	8.6	—	0.1	5.2	8.5
3	4.5	4.0	6.1	1.5	6.3	7.7	0.2	1.9	5.6	7.0
4	3.2	3.8	4.7	3.0	5.3	6.2	0.6	3.2	6.1	6.2
5	3.3	4.0	5.0	3.6	4.5	5.2	1.6	4.5	5.2	6.1
6	3.3	4.8	3.7	3.3	5.6	5.7	2.7	4.4	5.1	6.9
7	2.4	3.5	4.2	4.8	4.8	4.3	3.4	4.6	6.1	5.6
8	1.9	3.6	3.9	4.8	4.4	5.3	2.9	5.0	5.7	6.3
9	3.0	4.2	3.9	3.2	5.2	4.5	4.2	4.3	6.9	6.4
10	2.4	4.0	5.6	3.1	5.3	4.1	4.2	5.4	6.8	5.5
11	2.2	3.9	4.4	4.4	4.1	5.3	5.1	6.4	5.6	6.1
12	2.8	3.9	4.7	5.8	4.8	5.0	5.4	7.0	6.1	4.6
13	3.2	4.7	4.1	3.9	4.8	4.8	5.3	6.7	6.7	4.2
14	3.9	3.5	4.6	2.9	7.2	4.8	5.1	5.7	9.0	2.8
15	2.9	3.5	4.8	4.2	6.1	3.8	6.0	5.1	7.1	2.3
16	5.1	4.9	4.9	5.7	5.8	3.2	6.2	7.2	5.0	0.9
17	5.0	5.3	4.3	5.7	7.3	2.3	5.2	9.1	3.7	0.6
18	5.9	5.8	3.8	6.3	6.6	0.9	5.7	8.5	1.5	0.2
19	7.3	8.9	3.1	9.1	4.8	0.1	11.6	6.8	1.0	—
20	9.6	9.9	1.1	12.3	1.4	0.1	10.4	3.6	0.2	—
21	10.8	10.7	0.3	12.1	0.3	—	14.3	0.6	—	—
Total	2,623	2,216	2,216	1,450	1,450	1,450	887	887	887	887

families [15] the peak or mode is at age 6 and except for the high value (18.2) for 1-child families is remarkably constant. Indeed if the values of the percentages at age 6 for the different sized families be compared with the general mean 14.9, although the value for the 1-child family is well out of line, one finds that the 10 values as a whole, when regard is paid to their standard errors, cluster closely enough about the mean so that a Chi-square test fails to give a statistical inference ($P = 0.1$), and if the single value for the 1-child family be thrown out and the Chi-square test be applied to the remaining 9 values their chance to be more widely scattered is

[15] A broadening of the peak as the size of family increases may be observed in the Table with a tendency to bimodality in the larger families. However the percentage age distributions are not so important as the distribution on rates—if only we had a better rate-making basis!

TABLE 23

MEASLES. THE NUMBER AND PER CENT DISTRIBUTION BY AGE AND SIZE OF FAMILY FOR TOTAL CASES

Age	1-Child		2-Child		3-Child		4-Child		5-Child		6-Child		7-Child		8-Child		9-Child		10+-Child	
	Cases	Per Cent	Cases	Per Cent	Cases	Per Cent	Cases	Per Cent	Cases	Per Cent	Cases	Per Cent	Cases	Per Cent	Cases	Per Cent	Cases	Per Cent	Cases	Per Cent
<1	20	1.8	57	2.3	78	3.3	58	3.4	36	3.7	38	5.8	20	5.3	9	3.7	3	2.2	5	3.6
1	47	4.3	111	5.7	164	6.9	116	6.8	77	7.8	44	6.7	35	9.3	14	5.7	16	11.9	17	12.2
2	65	6.0	194	7.9	193	8.1	161	9.4	79	8.0	61	9.3	35	9.3	31	13.8	13	9.6	14	10.1
3	61	5.6	225	9.1	236	9.9	169	9.8	111	11.3	68	10.3	38	10.1	25	10.2	13	9.6	17	12.2
4	68	6.3	267	10.8	293	12.3	185	10.8	112	11.4	61	9.7	41	10.9	29	11.8	20	14.8	23	16.5
5	147	13.5	330	13.4	320	13.4	221	13.0	129	13.1	79	12.0	51	13.6	28	11.4	18	13.3	14	10.1
6	198	18.2	381	15.6	330	13.8	217	14.4	140	14.2	99	15.0	50	13.3	34	13.8	19	14.1	18	12.9
7	183	16.9	341	13.8	313	13.0	217	12.6	122	12.4	72	10.9	38	10.1	26	10.6	12	8.9	15	10.8
8	118	10.9	226	9.2	242	10.1	167	9.7	91	9.5	71	10.8	31	9.1	17	6.9	9	6.7	10	7.2
9	77	7.1	127	5.1	101	4.2	87	5.1	18	1.9	33	5.0	20	5.3	16	6.5	5	3.7	3	2.2
10	35	3.2	77	3.1	60	2.5	45	2.6	17	1.7	17	2.6	4	1.1	6	2.1	3	2.2	1	0.7
11	29	2.7	52	2.1	28	1.2	22	1.3	8	0.8	6	0.9	1	1.1	5	2.0	2	1.5	0	0.0
12-14	25	2.3	27	1.1	25	1.0	17	1.0	9	0.9	7	1.1	2	0.5	2	0.8	1	0.7	2	1.4
15-21	13	1.2	20	0.8	8	0.3	3	0.2	3	0.3	0	0.0	3	0.8	1	0.4	1	0.7	0	0.0
Total	1,086	—	2,468	—	2,391	—	1,718	—	985	—	659	—	375	—	246	—	135	—	139	—
Families	1,086	—	1,581	—	1,214	—	765	—	430	—	269	—	147	—	89	—	43	—	41	—
Cases/Fam.	1	—	1.56	—	1.97	—	2.25	—	2.29	—	2.45	—	2.55	—	2.76	—	3.14	—	3.39	—

$P = 0.8$. Thus the Chi-square test does not justify the inference that the group of percentages at age 6 really vary significantly; yet that inference is probably reasonably safe; the first difference from the mean 14.9 per cent is positive and subsequent ones are negative and with a down trend.

It is, however, not necessary to confine one's attention to the peak percentage or the percentages at earlier and at later years, one may apply Chi-square tests to the age distributions for each of the 10 sizes of family in comparison with the total distribution of cases by age or in comparison with one another; but it probably will be quite as illuminating to make the comparison graphically, plotting the percentages for the 1, 2, 3, 4–5, and 6+ families (Fig. 3). There is a definite tendency for the percentages at earlier ages to rise with size of family and apparently a less marked tendency for them to fall with it after the peak is passed.

24. *Rates of Incidence.*—The picture is quite different if one attempts to convert to relative rates by use of Table 19. If C_a is the number of cases at any age a and C is total number of cases, if P_a is population at age a and P is total population, each for a specified size of family, the ratio of the C_a/C to P_a/P will be the rate of incidence by age (viz., C_a/P_a) multiplied by the factor P/C which is independent of age. We have little confidence in this method of calculation but believe so thoroughly that rates should replace percentage distributions (possibly even poor estimates of rates should replace good percentage distributions) in our thinking about the fundamental epidemiological factors in the incidence of infectious diseases that we give rates in Table 24. The number of children in families of 1 to 5 children under twenty-one is given by the U. S. Census for Providence for 1930 as 11,511, 18,490, 18,201, 14,360, 10,525, and we estimate 17,300 in families of 6 children or more (we shall not attempt to correct to 1932). A comparison of Table 24 (Fig. 4) and Table 23 (Fig. 3) converted to the same groupings shows some striking differences: While the mode of the percentage distribution falls always at six years and the values of all the modal per-

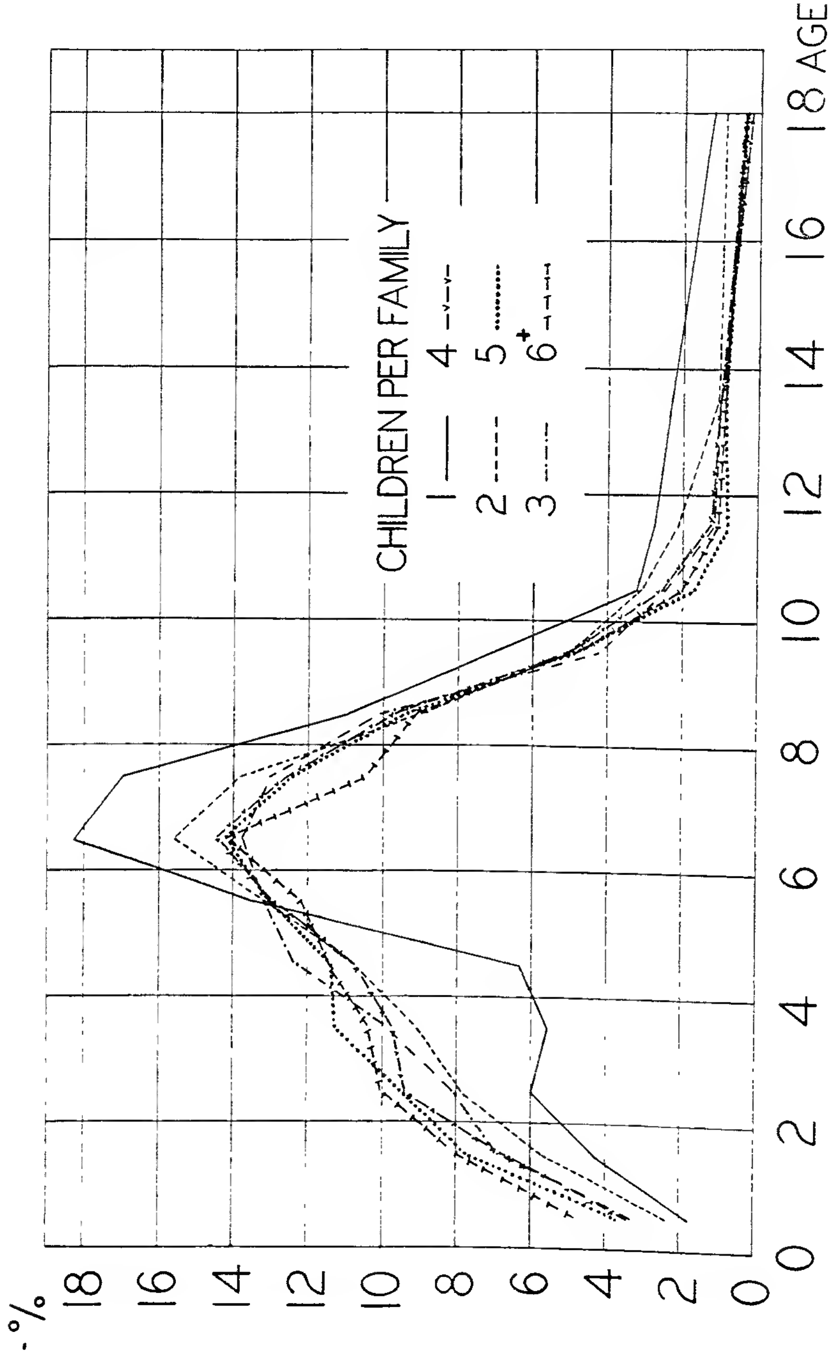

FIG. 3. Measles—percentage age distribution of total cases by family size. Age groups 12–14 and 15–21 were grouped and plotted as of the middle of the age group; this results in a distortion which raises the curve.

TABLE 24

MEASLES RATES PER THOUSAND BY AGE AND SIZE OF FAMILY BASED ON TOTAL CASES FOR THE YEARS 1929–34 AND THE AGE DISTRIBUTION OF THE BOSTON SAMPLE

Age	1	2	3	4–5	6+	Totals
<1	29	88	145	163	189	113
1	68	174	263	293	278	214
2	104	200	236	282	260	226
3	113	240	250	326	252	248
4	187	334	342	304	243	293
5	387	400	394	304	243	339
6	519	481	368	326	243	361
7	660	467	368	272	187	339
8	538	334	276	196	144	248
9	226	174	129	99	76	124
10	123	87	79	43	29	60
11	113	68	34	21	17	36
12–14	22	12	9	6	5	8
15–21	2.5	2.9	1.2	0.7	1.0	1.7
Total	94	133	131	109	90	113

centages lie in the range 13.8 to 18.2 about a mean of 14.9, the modes of the rates vary from one of 660 at 7 years of age for the 1-child family to one of 278 at 1 year of age for the family of 6 or more children. The variation of the modal frequencies is relatively much less than that of the modal rates both in age of incidence and in amount. Moreover the percentages under 1 all lay between 1.8 and 4.8 increasing with size of family, while the rates lie between 29 and 189, also increasing by size of family but showing a wider spread. The percentages at 5 years were very close together, ranging from 12.2 to 13.5, whereas the rates range from 243 to 400. On the rates the curve for families of 6+ children is nearly constant from 1 to 6 years varying only from 278 to 243 with a downward trend whereas on the percentage curve there is a rise from 8.1 to 14.2; for the 4–5 child family the rate curve is tolerably constant from 1 to 7 years, varying irregularly between 272 and 326, whereas for the percentages there is a rise from 7.1 to 14.3 (at 6) and a drop to 12.5. In a broad way one may say that the rates show much more clearly than

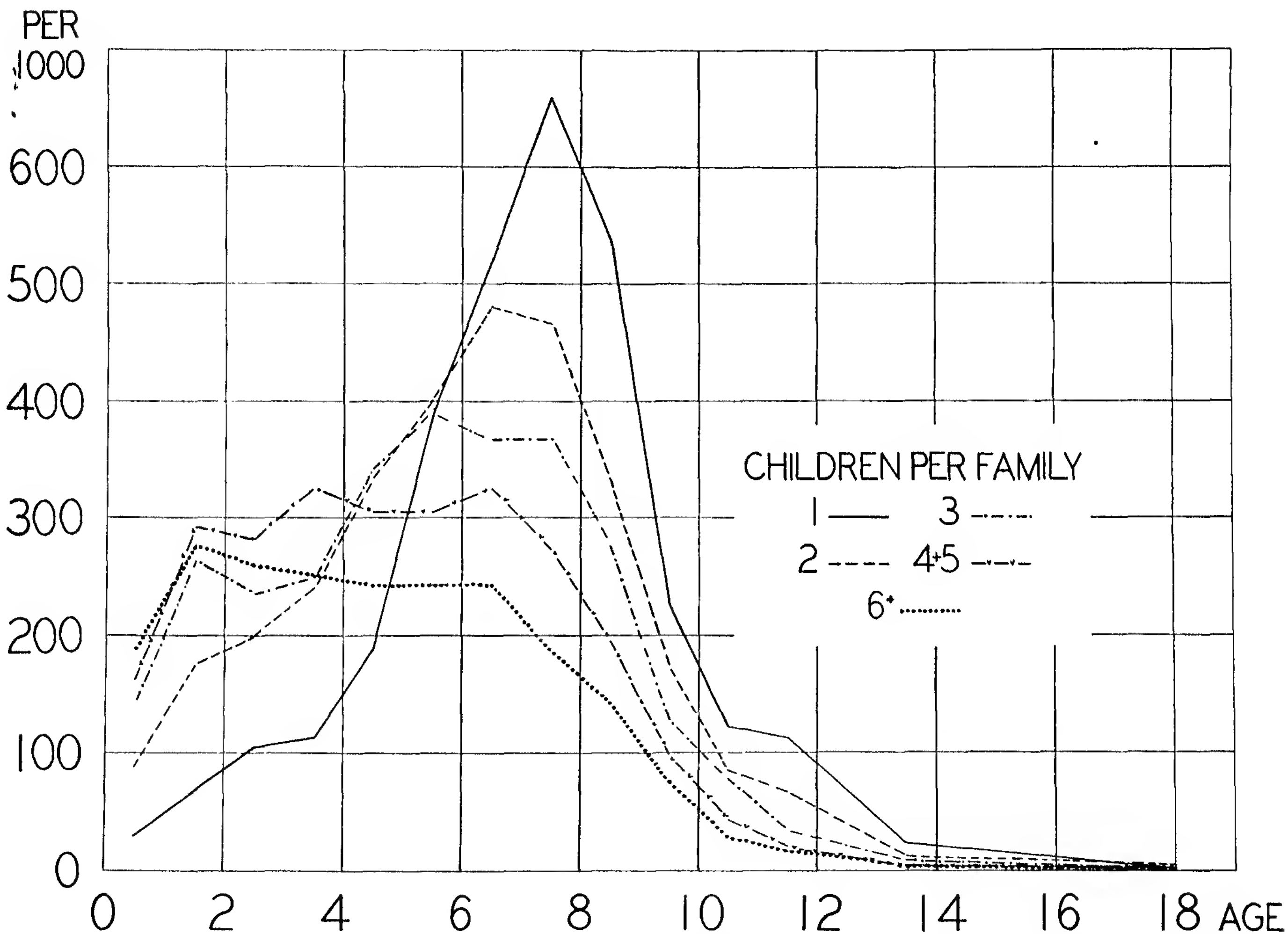

FIG. 1. Measles—rates for total cases by age and family size. Age groups 12–14 and 15–21 were grouped and plotted as of the middle of the age group

the percentages the tendency of measles to fall more heavily upon the younger children in the larger families. This is the sort of change that one finds in the urban-rural comparison—and may well be expected.

25. *Age Distribution and Rates for Primary Cases.*—When measles comes into the family it naturally spreads to the other children—which is one problem. Another problem is the introduction of measles into the family. We next give the percentage age distributions by size of family for primary cases and the corresponding rates per thousand based on the six years (Tables 25, 26; Figs. 5, 6). There are two types of comparison which may be made: (1) Percentages or rates for primary cases with those for total cases and (2) percentages with rates for the primary cases *inter se.* It has to be remembered that the primary cases are defined as the first and all subsequent cases in a period of one week and that this is not a strictly accurate classification of individual cases (though presumably statistically sound) and that the population basis for rates is not as satisfactory as one could wish. For such reasons elaborate statistical precision including a calculation

TABLE 25

MEASLES. PERCENTAGE DISTRIBUTION OF PRIMARY CASES BY AGE AND SIZE OF FAMILY (AND TOTAL CHILDREN INVOLVED)

Age	1	2	3	4–5	6 –	Totals
<1	1.8	1.8	2.0	2.3	2.6	2.0
1	4.3	4.1	3.6	3.4	4.2	3.9
2	6.0	5.7	5.3	5.8	5.1	5.6
3	5.6	7.5	7.5	6.3	7.3	6.9
4	6.3	8.6	9.4	7.7	8.0	8.1
5	13.5	13.1	13.2	12.9	11.8	13.0
6	18.2	18.5	16.9	19.7	21.3	18.7
7	16.9	16.4	18.2	17.8	17.0	17.3
8	10.9	11.0	13.8	12.8	12.5	12.2
9	7.1	6.3	4.9	6.1	6.7	6.1
10	3.2	3.2	2.6	2.7	1.8	2.8
11	2.7	2.1	1.1	1.2	0.5	1.6
12–14	2.3	0.9	1.2	1.2	0.9	1.2
15–21	1.2	0.9	0.4	0.3	0.3	0.6
Total	1,086	1,822	1,508	1,547	778	6,741

TABLE 26

MEASLES. PRIMARY CASE RATES PER THOUSAND BY AGE AND SIZE OF FAMILY FOR THE 6 YEARS 1929–34 USING BOSTON SAMPLE FOR POPULATION DISTRIBUTION

Age	1	2	3	4–5	6+	Totals
<1	29	50	57	62	49	48
1	68	94	91	81	72	82
2	104	108	99	106	67	97
3	113	148	116	112	90	119
4	187	197	166	118	85	142
5	387	286	249	174	117	224
6	519	424	282	255	180	306
7	660	414	331	218	148	291
8	538	296	232	149	99	201
9	226	158	91	68	49	97
10	123	66	51	29	13	43
11	113	50	20	13	4	25
12–14	22	7	7	4	2	6
15–21	2.5	2.5	1.0	0.6	0.5	1.3
Total	94	99	83	62	45	74.6

of standard errors seems unnecessary if not misleading and we may content ourselves with general descriptions based on graphs giving tabulated figures chiefly for comparisons that may be desirable in future studies or for the more analytical treatment that some may care to make.

Figure 5 shows that for each size of family the peaks of the percentages for primary cases come at 6 years, except for the 3-child family when it falls at 7. Whereas for total cases the modal percentage for the 1-child family was greatest with a general irregular downward tendency as family size increased, the modal percentages of primary cases tend to increase with size of family. On the whole so far as the eye can judge and taking account of the small number of primary cases in most of the groups it seems that there are not such striking differences between the percentage curves on size of family for primary cases (Fig. 5) as there were for total cases (Fig. 3); certainly there is not the striking progression of the percentages upward with size of family at the early ages. At the highest ages there seems to be the same tendency as

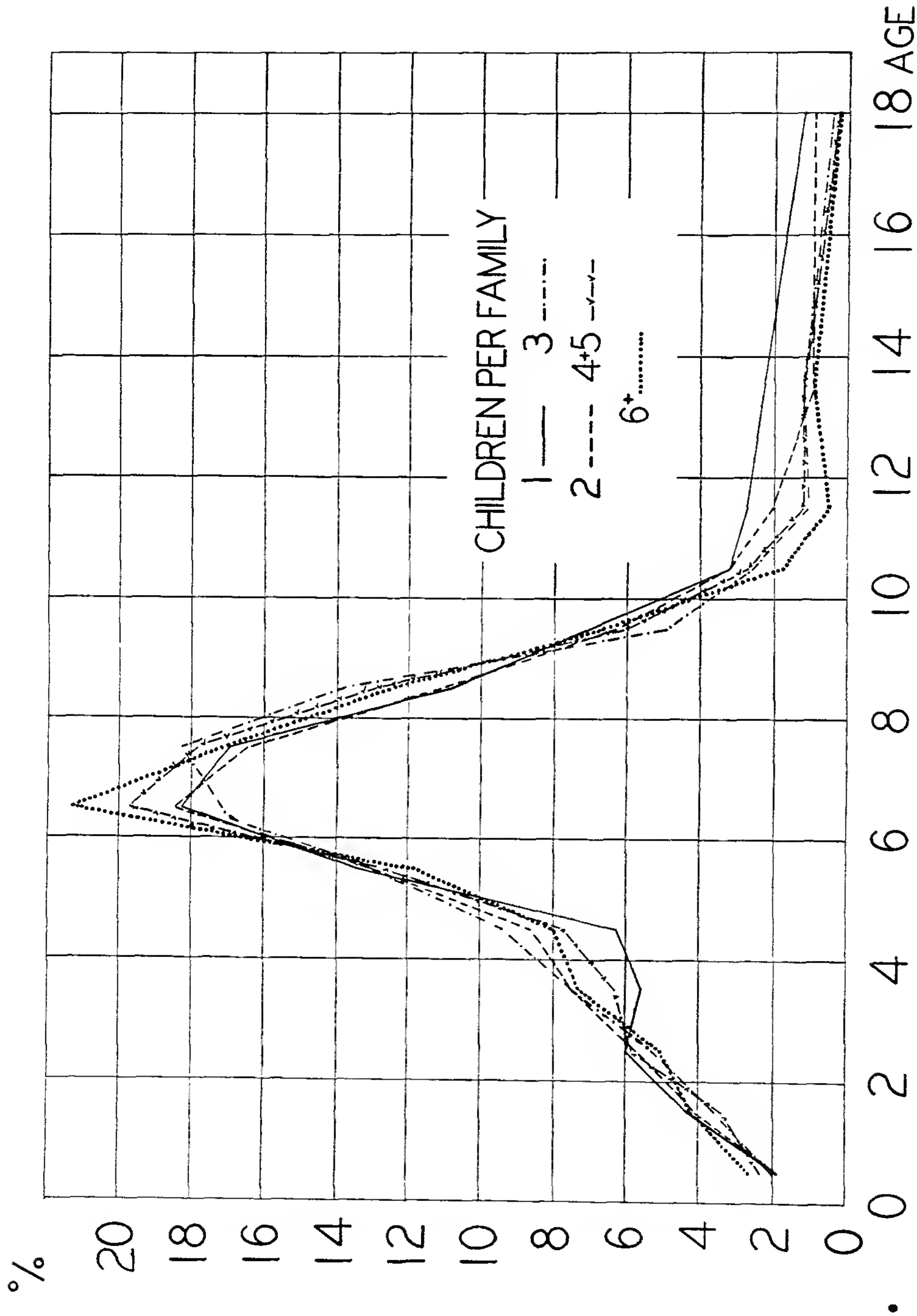

Fig. 5. Measles—percentage age disrribution of primary cases by family size. Age groups 12–14 and 15–21 were grouped and plotted as of the middle of the age group; thus introducing distortion.

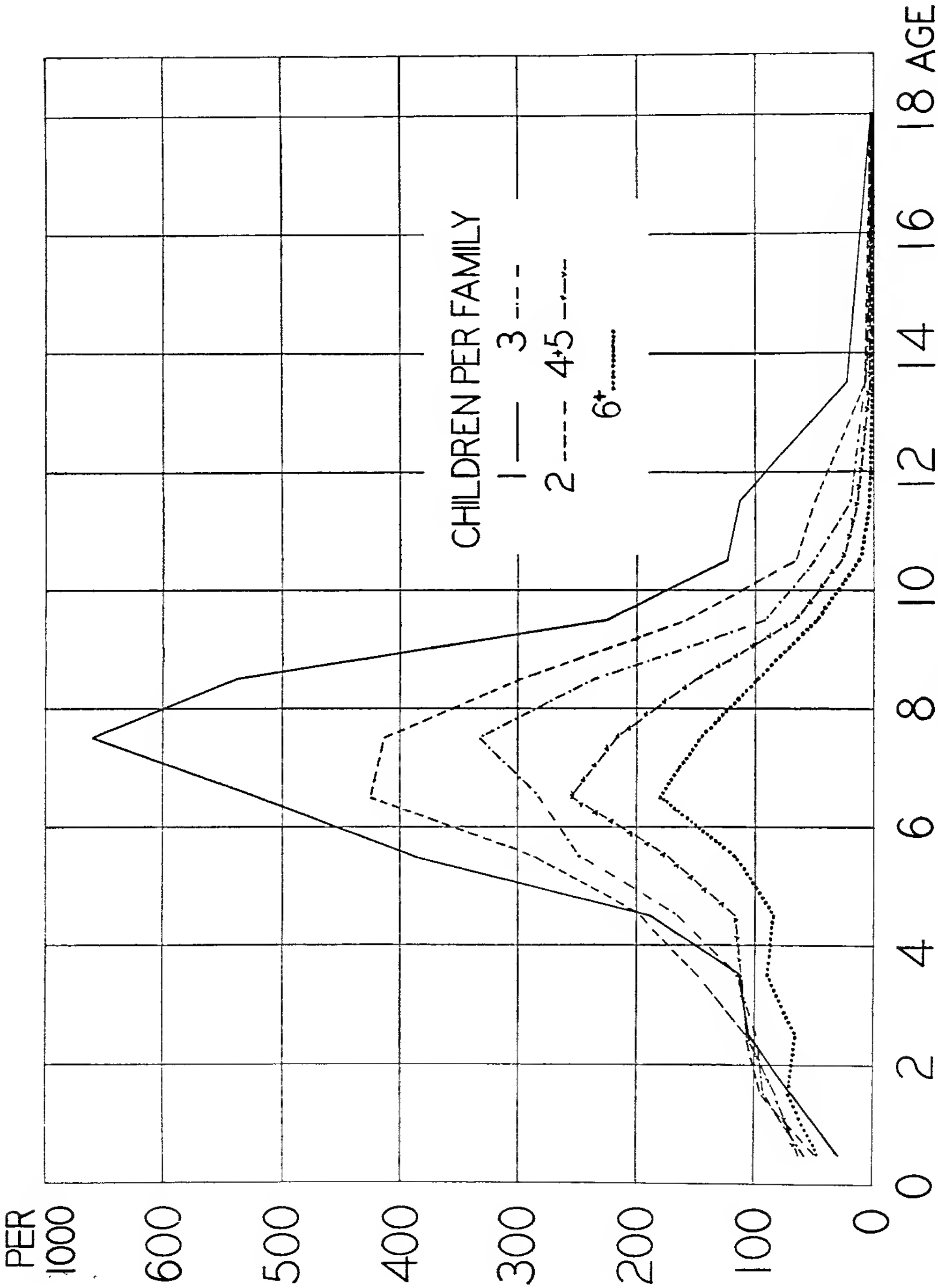

FIG. 6. Measles—rates for primary cases by age and family size. Age groups 12-14 and 15-21 were grouped and plotted as of the middle of the age group.

before for the percentages of cases to progress inversely to size of family. This constancy of pattern of the age distribution of primary cases, interesting as it is, may be of little real significance—it may be but a somewhat accidental result of a complex of opposing differences which could assume significance only if the complexes at different times and places worked in such a manner as to leave the phenomenon sufficiently constant to be a sort of empirical uniformity of descriptive value.

Indeed, if we turn to the comparison of the rates (Figs. 4, 6), poor as we recognize those rates to be, we see that the modal rates for primary cases all fall at 6 or 7 years instead of showing the backward regression from 7 to 1 as the family size increases which was seen for the rates for total cases. At all ages, with exceptions that seem insignificant, the rate of incidence of primary cases is less with increase of size of family. (This is natural because in the larger families some of the children will probably be immune.) A comparison of Figs. 5 and 6 shows the great differences which we find when comparing on percentages and on rates.

At this point it would be well by way of caution to stop long enough to compare the tables of rates of primary cases computed in the two ways sketched in Art. 21. In that place we gave the table for ages under five computed in one of the ways and here we have now the corresponding table computed in the other way. On the whole the rates as computed immediately above are larger than those computed by the other method. There are individual fluctuations from this tendency leading to differences which either on an absolute or on a relative basis are far from negligible, but apparently not of sufficient magnitude to disturb our general conclusions from the comparison of the graphs.

26. *Statistical Elements for Primary Cases.*—From the primary cases of measles by age (not exceeding 21) and by size of family there may be determined the mean and median ages, the standard deviation σ, the standard deviation of the mean, the upper and lower quartiles and the inter-quartile range.

If the distributions were normal the inter-quartile range would be 1.349σ; if there is a greater concentration of elements about the mean or median than on the normal curve the inter-quartile range will be less than 1.349σ. Table 27 gives the results.

TABLE 27

MEASLES. TOTAL PRIMARY CASES

	Size of Family										
	1	2	3	4	5	6	7	8	9	10+	Total
Mean Age..	6.65	6.40	6.33	6.36	6.42	6.22	6.35	6.26	6.05	6.07	6.40
σ. ..	2.93	2.75	2.55	2.59	2.41	2.54	2.59	2.33	2.39	2.11	2.65
σ mean	0.09	0.06	0.07	0.08	0.10	0.13	0.19	0.22	0.31	0.28	0.03
σ_σ	0.09	0.07	0.06	0.07	0.08	0.11	0.19	0.15	0.23	0.20	0.03
Q_1.... ...	5.07	4.69	4.71	4.88	5.05	4.56	5.09	4.78	4.50	4.92	4.81
Median. .	6.68	6.50	6.54	6.60	6.60	6.55	6.53	6.55	6.37	6.54	6.56
Q_3.	8.22	7.96	7.94	7.97	7.95	7.98	7.89	7.79	7.69	7.65	7.97
Q_3-Q_1.	3.15	3.27	3.23	3.09	2.90	3.42	2.80	3.01	3.19	2.73	3.16
1.349σ	3.95	3.71	3.44	3.49	3.25	3.43	3.49	3.14	3.22	2.85	3.57
Frequency.	1,086	1,822	1,508	1,001	546	363	185	114	60	56	6,741

The following remarks may be made. (1) The 1-child family has a mean age higher than the total by a significant amount; for no other size of family does the mean age depart from the general mean by a significant amount; on the whole there seems to be a slight decrease in mean age with increasing size of family although the mean ages for families of 2 to 8 children inclusive are remarkably steady. (2) The standard deviation for the 1-child family is significantly higher than for the total; the standard deviation of the 5- and 10+ child families are significantly below those for the total; on the whole there is a decrease of the standard deviation with size of family though the figure is reasonably constant for families with 2 to 7 children. (3) The median is always greater than the mean by irregular amounts of from 0.03 to 0.47 years; it shows less down trend than the mean and is in fact extremely stable about its mean of 6.56 having less than 40 per cent as much mean variation about that mean as the mean has about

its mean. (4) The lower quartile though irregular shows no downward tendency, the upper quartile which is otherwise very steady appears to show some downward trend with size of family. (5) The inter-quartile difference shows no downward trend; it is never greater than 1.349σ thereby indicating that there is a concentration of observations around the mean or median in excess of that on the normal curve. (6) The standard errors of the medians are not easy to estimate accurately but do not seem to be as large as would be given by the usual formula $\sigma_{\text{med.}} = 1.25\sigma_{\text{mean}}$ in the books. The question may be raised as to whether the median may not be as good a statistic of center for these distributions as the mean.

27. *Attack Rates.*—The attack rates by size of family as defined by Chapin and by Pope, viz., cases divided by all susceptibles (in the families attacked) including the cases themselves, may be tabulated by age. It is no use to tabulate the attack rate for the 1-child family because it is 100 per cent for all ages.

It is seen that the attack rate decreases on the totals with size of family. At individual ages the irregularity of the figures is such as to make it difficult to determine whether the attack rate does decrease with size of family but there seems to be a general though slight downward drift. As the attack rate for one-child families is 100 per cent and if the attack rates for other sizes of family do vary, the attack rate in the total child population (in measles families) by age must depend on the relative proportions of the families which go into the total. Moreover if the attack rates in any size of family vary with age as they certainly do, being low under 1 year and above 11 years and being highest in the ages around 7, any considerable differences in the age distribution of susceptibles in families of different sizes will make the total attack rates non-comparable unless they be adjusted to some standard age distribution. Such considerations would suggest a number of statistical refinements that might be applied at this point: that we do not stop to apply them is due to doubts as to the real significance of the attack rate. Thus the

low figures under one year of age reflect the immunity carried over by the children from their mothers—those who have not had a case are not truly susceptible except in a fraction (about one-half) of the cases (see Art. 10). The low figures over 11 years of age may reflect an increasing immunity among those who have not had measles (as would be the case for diphtheria or scarlet fever where much immunization seems to be acquired through sub-clinical infection) or if this is

TABLE 28

MEASLES. ATTACK RATES BY SIZE OF FAMILY AND BY AGE

Age	2-Child		3-Child		4-Child		5-Child		6+ Child	
	Susceptible	Rate	Susceptible	Rate	Susceptible	Rate	Susceptible	Rate	Susceptible	Rate
<1	102	55.9	162	48.1	128	45.3	86	41.9	160	46.9
1	158	89.2	189	86.8	130	89.2	83	92.8	145	86.9
2	211	91.9	217	88.9	172	93.6	84	94.0	173	90.8
3	245	91.8	252	93.7	187	90.4	115	96.5	182	88.5
4	291	91.8	312	93.9	195	94.9	119	94.1	197	89.8
5	342	96.5	238	94.7	237	94.5	138	93.5	203	93.6
6	392	98.0	344	95.9	256	96.5	148	94.6	231	95.2
7	344	99.1	332	94.3	224	96.9	127	96.1	173	94.2
8	235	96.2	250	96.8	179	93.3	98	95.9	153	92.2
9	133	95.5	113	89.4	98	88.8	56	85.7	96	80.2
10	83	92.8	68	88.2	51	88.2	23	73.9	38	81.6
11	58	89.7	32	87.5	31	71.0	15	53.3	30	56.7
12–14	38	71.1	32	78.1	28	60.7	23	39.1	42	33.3
15–21	30	66.7	19	42.1	13	23.1	10	30.0	27	18.5
Total	2,662	92.7	2,660	89.9	1,929	89.1	1,125	87.6	1,850	84.0

impossible for measles there remains the possibility that some of the older children reported as not having had measles have in reality had it (and indeed it is not infrequently observed that the fraction of children who are reported to have had measles before certain attained ages falls off for later years [16]) or finally there is the possibility suggested by Stockard [17] that the process of aging in some way increases resistance to the

[16] C. L. Scamman and C. R. Doering, "A Sickness Survey of Winchester, Mass.," *Am. Jour. Pub. Health*, **18**, 1264–1268, 1928

[17] C. R. Stockard, *Physical Basis of Personality*, p. 272.

diseases of childhood without necessarily implying active immunity in the immunological sense.

28. *Secondary Attack Rates.*—For the one-child family there is no secondary case and no secondary attack rate. For the 2-child, 3-child, 4+5-child, and 6+ child families the results are given in Table 29. On the totals there is very little evidence of decrease in the secondary attack rate with size of family. The individual secondary attack rates by age and size of family are subject to considerable sampling fluctuations due to small numbers and show irregularities which makes generalization difficult. The variations with age in each family seem not notably different from those in the total (Table 9); possibly at ages above 10 the two smallest family groups have higher secondary attack rates and the two largest groups lower rates than the totals, but the rates are unstable and one might perhaps say with equal assurance that under 5 years of age the secondary attack rates relative to those on the total were low for 2-child and high for 4+5-child families while irregular for 3-child and for 6+ child families. The age adjusted secondary attack rates for the different family groups are given and discussed in Art. 34.

TABLE 29

MEASLES. SECONDARY ATTACK RATES BY SIZE OF FAMILY AND BY AGE

Ages	Two-Child				Three-Child				4+5-Child				6+ Child			
	Primary Cases	Other Suscep	Sec Cases	Sec. Att	Primary Cases	Other Suscep	Sec Cases	Sec Att	Primary Cases	Other Suscep.	Sec Cases	Sec Att	Primary Cases	Other Suscep.	Sec. Cases	Sec Att
<1	32	70	25	35.7	30	132	48	36.4	36	178	58	32.6	20	140	55	39.3
1	75	83	66	79.5	55	134	109	81.3	52	161	141	87.6	33	112	93	83.0
2	103	108	91	84.3	80	137	113	82.5	89	167	151	90.4	40	133	117	88.0
3	137	108	88	81.5	113	139	123	88.5	97	205	183	89.3	57	125	104	83.2
4	157	134	110	82.1	141	171	152	88.9	119	195	178	91.3	62	135	115	85.2
5	239	103	91	88.3	199	139	121	87.1	199	176	154	87.5	92	111	98	88.3
6	337	55	47	85.5	255	89	75	84.3	304	100	83	83.0	166	65	54	83.1
7	298	46	43	93.5	274	58	39	67.2	276	75	63	84.0	132	41	31	75.6
8	201	34	25	73.5	208	42	34	81.0	198	79	63	79.7	97	56	44	78.6
9	114	19	13	68.4	74	39	27	69.2	95	59	40	67.8	52	44	25	56.8
10	59	24	18	75.0	39	29	21	72.4	42	32	20	62.5	14	24	17	70.8
11	38	20	14	70.0	16	16	12	75.0	18	28	12	42.9	4	26	13	50.0
12–14	16	22	11	50.0	18	14	7	50.0	18	33	8	24.2	7	35	7	20.0
15–21	16	14	4	28.6	6	13	2	15.4	4	19	2	10.5	2	25	3	12.0
Totals	1,822	840	646	76.9	1,508	1,152	883	76.6	1,547	1,507	1,156	76.7	778	1,072	776	72.4

29. *Immune Rates.*—It is of some interest to take all contacts exposed to the primary cases in the family and tabulate the number of them who are said to have had the disease (and do not contract it). The ratio of the latter to the former expressed as a percentage will give for the measles families the percentage of immunes by age among the contacts. Whether this is a fair index of the immune rate in the general population may be doubted. There is, obviously, for the measles families a discontinuity in the immune rate across the epidemic; for before the epidemic the rate is determined as stated, but after the epidemic there are added to these immunes among the contacts all the primary cases (who survived) so that for these families the immune rate is increased by the epidemic. However, for any epidemic the number of families into which measles comes is not a large fraction of the whole, varying perhaps from 8 per cent for 1-child families to 25 per cent for 9+ child families in a major epidemic (Table 17 gives the rate per cent for the six year period which contained one major epidemic with about 6/7 of the cases), and so the discontinuity in the population as a whole is far from as large as would appear in the measles families alone. There is, of course, the further difficulty that the contacts in measles families may not be a fair sample relative to immunity for the whole population, even though the most reasonable estimates be applied to allow for the discontinuity across the epidemic in the measles families themselves.

Table 30 shows that the immune rates increase with age, as would be expected, in families of each size and also that at each age the rates increase with size of family, as again might be expected. We have found no other series with which to compare these so that we cannot estimate how typical these rates may be. In all sizes of families they run up well into the 90's for the group 15–21. Ninety-seven per cent of all contacts 15–21 years old in measles families have had the disease. As there were 131 who were said to have had measles but had it again and have been left out of the count above, we may say that over 98 per cent of all contacts are said to

have had measles by the time they are in the 15–21 age group when measles comes into the family—a figure considerably higher than is found in health surveys when the population is canvassed,[16] but more in line with the beliefs of the medical or public health profession.

TABLE 30

MEASLES. IMMUNES AND IMMUNE RATES PER CENT BY SIZE OF FAMILY AND BY AGE

Age	2-Child		3-Child		4–5-Child		6+ Child		Total	
	Immune	Rate	Immune	Rate	Immune	Rate	Immune	Rate	Immune	Rate
<1	0	0	1	1	0	0	0	0	1	0.2
1	2	2	2	1	1	1	1	1	6	1.2
2	0	0	5	4	4	2	3	2	12	2.2
3	4	4	6	4	10	5	7	5	27	4.5
4	7	5	12	7	20	9	18	12	57	8.2
5	13	11	20	13	30	15	38	26	101	16.0
6	23	29	32	26	43	30	44	40	142	31.5
7	25	35	35	38	88	54	89	68	237	51.9
8	20	37	54	56	127	62	98	64	299	58.6
9	28	60	59	60	145	71	149	77	381	70.3
10	42	64	79	73	200	86	196	89	517	82.6
11	43	68	69	81	166	86	189	88	467	83.8
12–14	87	80	230	94	534	94	592	94	1,443	93.3
15–21	206	94	378	97	788	98	891	97	2,263	97.0
Total	500	37.3	982	46.0	2,156	58.9	2,315	68.3	5,953	56.6

SCARLET FEVER BY SIZE OF FAMILY AND BY AGE

30. *Percentage Age Distribution.*—We shall not consider separately total cases and primary cases in Scarlet Fever because of the small number of secondary cases. In measles we had 6741 primary cases (with a period of a week allowed for multiple primaries) and 3461 secondary cases; in scarlet fever we have 2439 primary cases (with no period allowed for multiple primaries [9]) and only 630 secondaries.

The percentage age distribution (Table 31, Fig. 7) shows less stability than it did for measles (Table 23, Fig. 3) as may be expected from the smaller number of cases if for no other reason. One cannot see the distinct progression upward

TABLE 31

SCARLET FEVER. NUMBER AND PERCENTAGE AGE DISTRIBUTION OF ALL CASES BY AGE AND BY SIZE OF FAMILY

Family	1		2		3		4–5		6+		Total	
Age	Cases	Per Cent	Cases	Per Cent	Cases	Per Cent	Cases	Per Cent	Cases	Per Cent	Cases	Per Cent
<1	1	0.3	6	0.8	4	0.5	4	0.5	3	0.8	18	0.59
1	6	1.7	26	3.3	13	1.7	27	3.5	10	2.6	82	2.67
2	17	4.9	48	6.0	36	4.8	44	5.6	17	4.4	162	5.28
3	18	5.1	61	7.6	46	6.1	61	7.8	31	8.1	217	7.07
4	26	7.4	74	9.3	66	8.8	59	7.5	30	7.8	255	8.31
5	44	12.6	85	10.6	89	11.8	82	10.5	36	9.4	336	10.95
6	58	16.6	121	15.1	99	13.1	110	14.1	42	10.9	430	14.01
7	40	11.4	101	12.6	101	13.4	86	11.0	48	12.5	376	12.25
8	28	8.0	73	9.1	66	8.8	79	10.1	33	8.6	279	9.09
9	25	7.1	54	6.8	61	8.1	61	7.8	38	9.9	239	7.79
10	25	7.1	38	4.8	43	5.7	45	5.8	18	4.7	169	5.51
11	13	3.7	22	2.8	31	4.1	37	4.7	19	4.9	122	3.98
12–14	28	8.0	66	8.3	63	8.4	52	6.6	38	9.9	247	8.05
15–21	21	6.0	24	3.0	36	4.8	35	4.5	21	5.5	137	4.46
Total	350	—	799	—	754	—	782	—	384	—	3,069	—
Families	350	—	698	—	591	—	537	—	241	—	2,417	—
Cases/Fam.	1.0	—	1.14	—	1.28	—	1.46	—	1.59	—	1.27	—

of the percentages at the lower ages as the size of the family increases and the order is not clear by size of family at the highest ages. What is obvious is that scarlet fever for all sizes of families is running higher than measles at the higher ages. On the whole the curves for age distribution of total cases of scarlet fever seem to be acting rather as the curves for primary cases of measles (Table 25, Fig. 5) than as those for total cases.[18]

31. *Rates of Incidence.*—If we resort to the same basis of computing rates as for measles (which however inadequate for absolute determinations may be adequate for comparative purposes) we find Table 32 (Fig. 8). There may be some slight evidence of a progression of mode backward on age

[18] This would be an interesting observation to examine if we had enough cases of scarlet fever, primary and secondary individually, to make possible a detailed statistical stable comparison with measles.

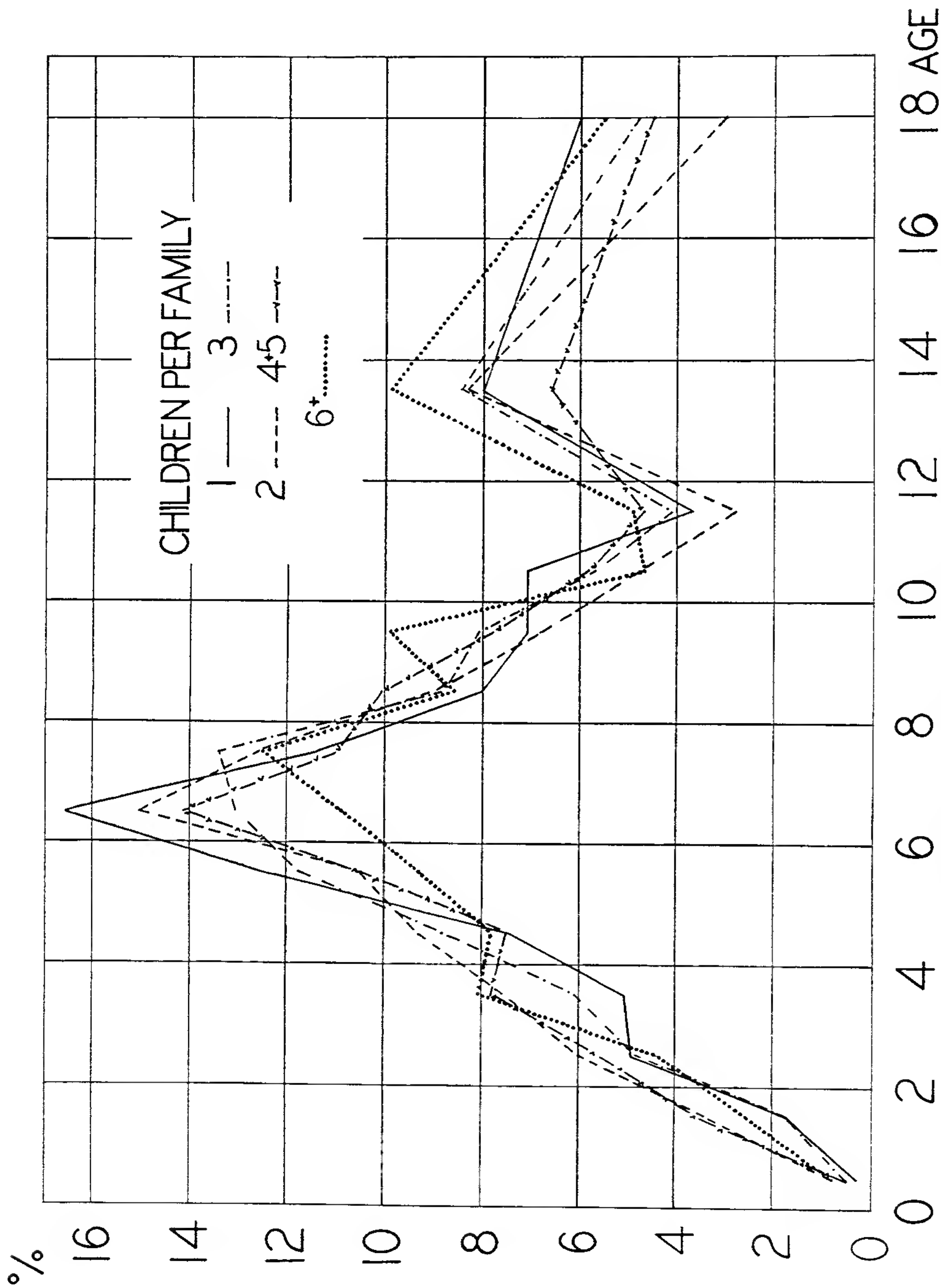

FIG. 7. Scarlet fever percentage age distribution of total cases by family size. Age groups 12-14 and 15-21 were grouped and plotted as of the middle of the age group; thus producing distortion—the curves really fall off.

TABLE 32

SCARLET FEVER RATES PER THOUSAND BY AGE AND SIZE OF FAMILY BASED ON TOTAL CASES AND THE AGE DISTRIBUTION OF THE BOSTON SAMPLE

Family	1	2	3	4–5	6–	Total
<1	2	10	7	7	8	6
1	9	33	21	41	22	26
2	28	52	46	50	29	44
3	33	65	50	69	49	58
4	70	95	75	60	40	68
5	116	104	112	72	47	85
6	152	151	112	91	47	102
7	146	138	120	69	56	95
8	128	108	75	60	33	68
9	73	73	79	44	38	58
10	91	43	58	31	17	37
11	52	29	37	26	19	29
12–14	25	28	24	12	12	18
15–21	4	4	6	5	4	4
Total	30.4	43.2	41.4	31.4	22.2	34.0

with size of family (indicated not so much by actual progression of the mode as by the behavior of the curves as a whole at lower ages) as in the case of measles for all cases (Table 24, Fig. 4) but the likeness to the curves for the primary cases (Table 26, Fig. 6) seems greater. Comparing all cases of scarlet fever and primary cases of measles we note that the modal rates of the former range from 56 for the 6+ family to 152 for the 1-child family whereas for the latter the rates range from 180 to 660, the percentage range being less for scarlet fever. The modal rates for the 1- and 2-child families are nearly the same for scarlet fever but greatly different for measles.

32. *Statistical Elements for Primary Cases.*—To compare with the results for measles (Table 27) the table of statistical elements (Table 33) was made up on primary cases only. We may observe: (1) There seems to be no outstanding mean age nor any trend. (2) There seems to be nothing notable about the standard deviation. (3) The mean ages are running higher than for measles by about 20–25 per cent; the standard deviations are running higher than for measles by nearly 40

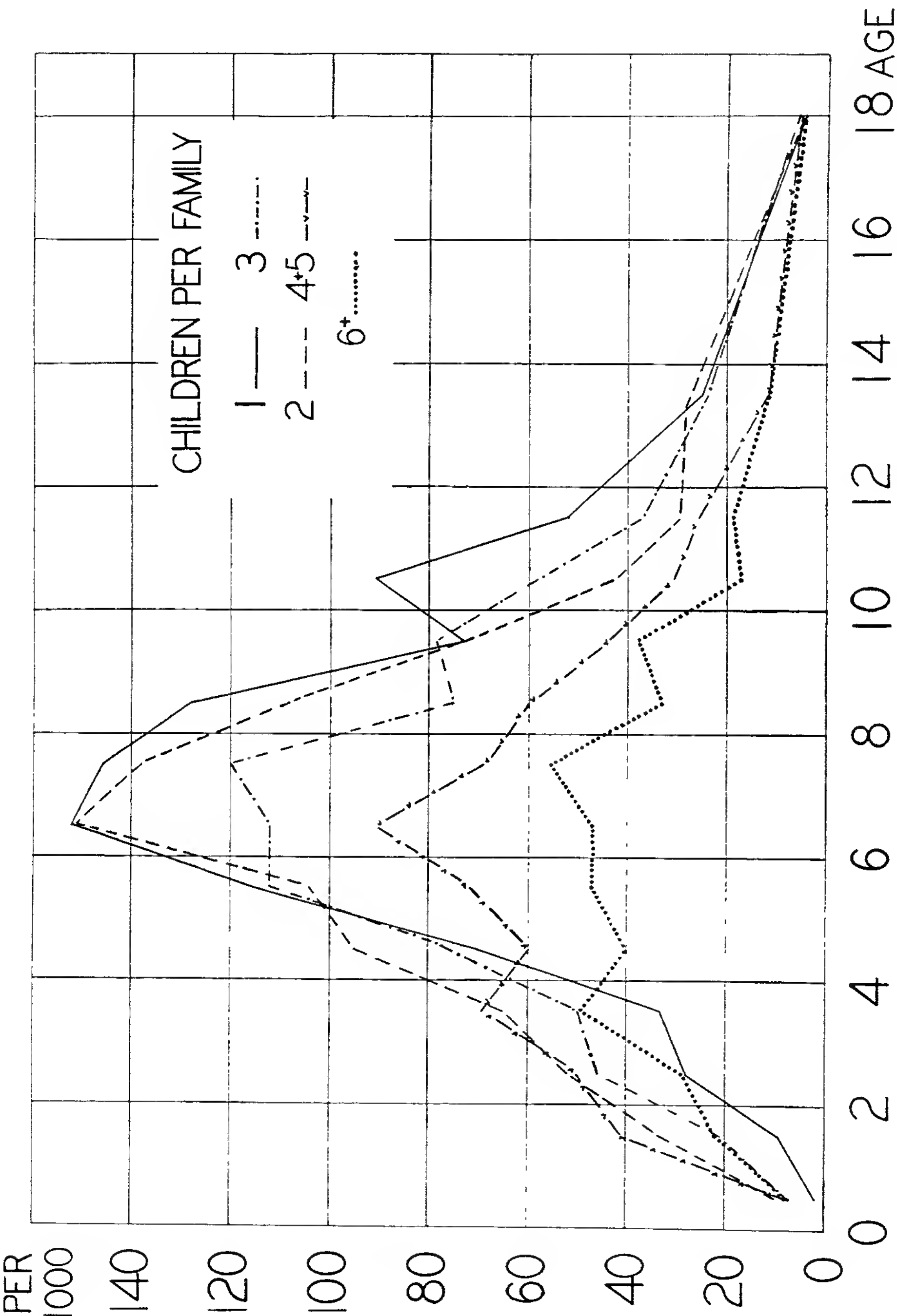

FIG. 8. Scarlet fever—rates for total cases by age and family size. Age groups 12-14 and 15-21 were grouped and plotted as of the middle of the age group.

per cent; the coefficient of variation is therefore running higher for scarlet fever than for measles by around 10–15 per cent. (4) The median is uniformly less than the mean (with the exception of family size 10+ where numbers are small) instead of greater as for measles; the median is, however, for scarlet fever less stable than the mean. (5) The lower quartile is irregular with no downward tendency and the same is true for the upper quartile. (6) The inter-quartile range is on the whole less than 1.349σ; the two exceptions being in the two larger families where the numbers are small; on the whole the excess of 1.349σ over Q_3-Q_1 is slightly greater for scarlet fever than for measles but the difference is almost certainly not significant.

TABLE 33

SCARLET FEVER. TOTAL PRIMARY CASES

	Size of Family										Total
	1	2	3	4	5	6	7	8	9	10–	
Mean Age.	7.95	7.50	8.04	7.87	8.19	8.42	7.81	8.34	7.64	7.39	7.86
σ	3.87	3.56	3.58	3.59	3.85	3.90	3.07	3.51	4.58	3.78	3.68
σ mean. .	0.21	0.13	0.15	0.19	0.29	0.37	0.42	0.57	1.00	0.89	0.07
σ_J	0.19	0.11	0.13	0.17	0.26	0.28	0.28	0.53	0.54	0.60	0.07
Q_1	5.45	5.17	5.59	5.67	5.86	5.90	5.67	6.00	3.94	4.67	5.46
Median Age	7.14	7.00	7.45	7.44	7.55	7.63	7.56	7.72	6.67	8.00	7.29
Q_3 . . .	10.00	9.34	9.93	9.88	10.03	11.00	9.78	10.00	11.63	10.00	9.80
Q_3-Q_1. . .	4.55	4.17	4.34	4.21	4.17	5.10	4.11	4.00	7.69	5.33	4.34
1.349σ .	5.22	4.80	4.83	4.84	5.19	5.26	4.14	4.73	6.18	5.10	4.96
Frequency .	350	702	599	366	177	114	54	38	21	18	2,439

33. *Attack Rates.*—Although it is doubtful whether attack rates are of much significance in themselves they are so widely used that for comparative purposes we give them in Table 34. Whereas for measles (Table 28) the attack rates rose abruptly from a figure around 50 per cent for children under one year of age to a high plateau in the eighties and nineties which was well maintained up to 10 years, for scarlet fever the attack rates rise gradually and fall away so that a mode at 6 years is well defined. For measles the attack rates dropped off

from 92.7 to 84.0 in going from the 2-child to the 6+ child family; in scarlet fever they fall off from 59.7 to 24.3. The general phenomena of a decrease of the attack rate at the higher ages and of a decrease at a given age with increasing size of family are common to both diseases, except that the latter phenomenon appears more definitely existent at each age (despite the smaller number of cases) in scarlet fever than in measles.

TABLE 34

SCARLET FEVER ATTACK RATE BY SIZE OF FAMILY AND BY AGE

Age	2-Child		3-Child		4+5 Child		6+ Child	
	Susceptible	Rate	Susceptible	Rate	Susceptible	Rate	Susceptible	Rate
<1	39	15.4	51	7.8	89	4.5	67	4.5
1	69	37.7	62	21.0	88	30.7	43	23.3
2	87	55.2	104	34.6	124	35.5	71	23.9
3	102	59.8	104	44.2	129	47.3	76	40.8
4	126	58.7	130	50.8	139	42.4	84	35.7
5	113	75.2	156	57.1	155	52.9	86	41.9
6	155	78.1	157	63.1	192	57.3	100	42.0
7	142	71.1	167	60.5	179	48.0	97	49.5
8	101	72.3	120	55.0	168	47.0	90	36.7
9	80	67.5	120	50.8	149	40.9	94	40.4
10	68	55.9	86	50.0	126	35.7	86	20.9
11	44	50.0	73	42.5	108	34.3	93	20.4
12–14	119	55.5	155	40.6	241	21.6	247	15.4
15–21	94	25.5	187	19.3	299	11.7	349	6.0
Total	1,339	59.7	1,672	45.1	2,186	35.8	1,583	24.3

34. *Secondary Attack Rates.*—Table 35 gives secondary attack rates by age and size of family. The crude total rates are the same for the first two groups of families but lower for the families of 6 children and up. This was precisely their course for measles (Table 29). By age the secondary attack rate rises from a low figure (3 per cent) for under 1 year to a maximum of between 20 per cent and 30 per cent at ages 3 to 7 and falls away again to about 3 per cent at the highest ages (15–21); whereas for measles the rise was from 35 per cent to between 80 per cent and 90 per cent in the age groups 1 to 7, falling away to around 15 per cent at the highest group. For scarlet fever as for measles there seems to be irregular

variability with size of family. We have to recognize that the crude rates on totals are not strictly comparable because of the different rates at different ages and the different age distributions of the susceptibles. When there are such differences and when because of small numbers the individual rates vary so much as to make it difficult to see what if any trend there is, one often has recourse to the device of "adjustment."

TABLE 35

SCARLET FEVER. SECONDARY ATTACK RATE PER CENT BY SIZE OF FAMILY AND BY AGE

Age	2+3 Child				4+5 Child				6+ Child			
	Primary Case	Other Susc	Sec Case	Sec Rate	Primary Case	Other Susc	Sec Case	Sec Rate	Primary Case	Other Susc	Sec Case	Sec Rate
<1	7	83	3	3.6	0	89	4	4.5	1	66	2	3.0
1	24	107	15	14.0	16	72	11	15.3	5	38	5	13.2
2	54	137	30	21.9	27	97	17	17.5	9	62	8	12.9
3	78	128	29	22.7	30	99	31	31.3	16	60	15	25.0
4	111	145	29	20.0	26	113	33	29.2	17	67	13	19.4
5	143	126	31	24.6	52	103	30	29.1	24	62	12	19.4
6	197	115	23	20.0	88	104	22	21.2	32	68	10	14.7
7	175	134	27	20.1	71	108	15	13.9	33	64	15	23.4
8	128	93	11	11.8	54	114	25	21.9	18	72	15	20.8
9	96	104	19	18.3	48	101	13	12.9	26	68	12	17.6
10	78	76	3	3.9	36	90	9	10.0	11	75	7	9.3
11	44	73	9	12.3	32	76	5	6.6	15	78	4	5.1
12–14	114	160	15	9.4	37	204	15	7.4	25	222	13	5.9
15–21	52	229	8	3.5	26	273	9	3.3	13	336	8	2.4
Total	1,301	1,710	252	14.7	543	1,643	239	14.5	245	1,338	139	10.4

If we use the general population of all susceptibles in all the families to give a standard age distribution of susceptibles and adjust the rates for the different family groups to that population, we find:

	Measles				Scarlet Fever		
	2	3	4+5	6+	2+3	4+5	6+
Crude	76.9	76.6	76.7	72.4	14.7	14.5	10.4
S.D	±1.5	±1.2	±1.1	±1.4	±0.9	±1.0	±0.8
Adj.	79.0	77.5	79.1	76.5	14.5	15.6	13.5

From the tabulation of the results it is seen that although the values of the crude rates for the smaller families are alike in both measles and scarlet fever and differ from the values for the largest families (6+) by more than twice the standard deviation of the difference, the values on adjustment are much nearer together, and pursue an irregular course rising slightly for medium sized families as compared with the smallest and falling away for the largest group. There is certainly no significant simple down trend of the secondary attack rate with size of family, and the differences between the rates seem not significantly large in view of their probable errors, nevertheless one is on the whole tempted to infer that the secondary attack rate for families of 6 or more children is less than for smaller families.

35. *Immune Rates.*—Table 36 gives for scarlet fever the ratio per cent of immunes among total contacts of the primary case. The numbers are small and the rates are irregular. They are of course much lower than for measles (Table 30).

TABLE 36

SCARLET FEVER. IMMUNES AND IMMUNE RATES PER CENT BY SIZE OF FAMILY AND BY AGE

Age	2–3 Child		4–5 Child		6+ Child		Total	
	Immune	Rate	Immune	Rate	Immune	Rate	Immune	Rate
<1	0	0.0	0	0.0	0	0.0	0	0.0
1	0	0.0	0	0.0	0	0.0	0	0.0
2	1	0.7	0	0.0	0	0.0	1	0.3
3	3	2.3	4	3.9	4	6.2	11	3.7
4	4	2.7	5	4.2	4	5.6	13	3.8
5	11	8.0	7	6.4	5	7.5	23	7.3
6	15	11.5	4	3.7	3	4.2	22	7.1
7	9	6.2	9	7.7	12	15.8	30	8.9
8	16	14.7	11	8.8	12	14.3	39	12.3
9	7	6.3	12	10.6	10	12.8	29	9.6
10	18	19.1	14	13.5	7	8.5	39	13.9
11	6	7.6	7	8.4	7	8.2	20	8.1
12–14	30	15.8	25	10.9	25	10.1	80	12.0
15–21	38	14.2	44	13.9	39	10.4	121	12.6
Total	158	8.5	142	8.0	128	8.7	428	8.4

There seems to be an increase to above 12.5 per cent, or one in eight, with age. There is no clear indication that at each age the rates increase with size of family as they did with measles.[19]

Measles in the Two-Child Family

36. *Cases, Immunes and Susceptibles.*—The one-child measles family seems to need no analysis other than that given above by the comparisons according to size of family; with the two-child family we may begin the study of a considerable number of interesting points relative to the behavior of measles

TABLE 37

Measles. Age Distribution of Single Primaries, Double Primaries, Secondaries, Immunes, and Susceptible Escapes in the Two-Child Family

Age	Single Primaries	Double Primaries	Secondaries	Immunes	Susceptible Escapes
<1	14	18	25		45*
1	44	31	66	2	17
2	60	43	91		17
3	72	65	88	4	20
4	97	60	110	7	24
5	173	66	91	13	12
6	260	77	47	23	8
7	248	51	43	25	3
8	172	29	25	20	9
9	90	23	13	28	6
10	49	10	18	42	6
11	33	5	14	43	6
12–14	13	3	11	87	11
15–21	15	1	4	206	10
Total	1,340	482	646	500	194

* Of these something like 7/12 would probably really be immunes.

[19] For scarlet fever with its large number of subclinical cases, or of immunity acquired without a clinical case, as with diphtheria, the whole epidemiological picture would change if we had a true classification of susceptibles and immunes based on a reliable test. Only in such a way could a really sound comparison with measles be made. The best treatment of this sort that we know is that of Gordon, Badger, Darling and Schooten, "Reaction of Familial Contact to Scarlet Fever Infection," *Amer. Jour. Public Health*, 25, 531–544, 1935. A large amount of Dr. Gordon's material from his prolonged studies on scarlet fever in Detroit, and more recently in Romania, has not yet been published.

(or of scarlet fever) within the family. Recalling that we have in measles a period of a week for double primaries we may tabulate the age distribution of single primaries, double primaries, secondaries, immunes and susceptible escapes.

The mean age of single primaries is 6.77, that of double primaries is 5.37, and that of secondary cases is 4.91. If we use the standard deviation $\sigma = 2.75$ of all primary cases as a good enough approximation to that of each group of (single or double) primaries, the standard deviation of the means would be 0.15 and the difference of 1.40 would be significant. Entirely apart from the difference in the means it is clear that the proportion of double primaries at the lower ages (under 5 years) is greater than that of single primaries whereas the proportion of double primaries from 6 years on is less than that of single primaries so that the age distributions are markedly different. The distribution of the secondaries is even more markedly concentrated in the lower ages, albeit that in the ages over 10 years there is also an excess of secondaries as compared with double primaries. It is in the years of maximum incidence of measles, viz., 5, 6, 7, that the proportions of secondaries are below those of double primaries.

37. *Various Age Correlations.*—There are five correlation tables showing the distributions of ages of pairs of children for these two-child families: 38*a*, giving the double primaries, which has been tabulated with the younger cases against the older, but might have been tabulated symmetrically; 38*b*, giving the single primaries against their sibling, in which the totals are separated into younger and older, counting each of a pair of the same age as half younger and half older; 38*c*, giving the single primaries against their secondaries, for those families in which there was a secondary, with the marginal totals separated into older and younger siblings; 38*d*, giving the primaries against the susceptible escapes, where there were such; 38*e*, giving the primaries against the immunes, where there were such. Table 38*b* is simply the sum of Tables 38*c*, *d*, *e*; various other tables might be had by other summa-

tions with or without symmetrizing.[20] Thus the problem of age distribution becomes two dimensional and complicated in studying the detail of the two-child family. The numbers are not large enough to justify reduction to percentages and as the same was true of Table 21 giving (on the basis of the Boston sample) the age distribution of pairs of children in the 2-child family, the question of rates for each of the cells in Tables 38 is hopeless; yet it is the rates that one needs for the most satisfactory theoretical treatment. However, such rates could not be available in most health districts because of the small total population, whereas certain calculations upon the figures themselves could be made for comparative purposes.

38. *Comparison of Mean Ages of Primaries.*—The mean ages of older and younger of the double primaries are 6.74 and 4.00 whereas the average age of all double primaries is

TABLE 38*a*

MEASLES AGES OF CHILDREN WHO WERE DOUBLE PRIMARIES, YOUNGER VERTICAL, OLDER HORIZONTAL

Age	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	Total Younger
<1	1	5	4	1	4	1	1	—	—	1	—	—	—	—	—	18
1		4	12	4	4	3	—	3	—	—	—	—	—	—	—	30
2			6	7	11	5	1	1	2	1	—	—	—	—	—	34
3			1	5	11	19	2	4	—	—	—	—	—	—	—	42
4				2	6	16	11	4	—	—	2	—	—	—	—	41
5					1	7	11	3	4	1	1	1	—	—	—	29
6							8	7	8	3	—	—	—	—	—	26
7							2	2	5	2	2	1	—	—	1	15
8									3	1	—	1	—	—	—	5
9										1	—	—	—	—	—	1
Total Older	1	9	23	19	37	51	36	24	22	10	5	3	0	0	1	241

[20] If m_x, m_y, σ_x, σ_y, r are the means, standard deviations and correlation coefficient of two variables (in an asymmetrical table) the means M, standard deviations Σ, and correlation R in the symmetrical table formed therefrom are respectively

$$M = \frac{m_x + m_y}{2}, \qquad \Sigma = \left[\frac{\sigma_x^2 + \sigma_y^2}{2} + \frac{(m_x - m_y)^2}{4}\right]^{1/2},$$

$$R = \frac{r\sigma_x\sigma_y - \frac{1}{4}(m_x - m_y)^2}{\frac{1}{2}(\sigma_x^2 + \sigma_y^2) + \frac{1}{4}(m_x - m_y)^2}.$$

TABLE 38*b*

MEASLES. TWO-CHILD FAMILY WITH ONE PRIMARY CASE, AGES OF SIBLINGS

Age of Primary Case	Age of Sibling																						Primary Cases		
	<1	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	Total	Older	Younger
<1		1	1	1	1			2	1		4		1							2			14		11
1	2		1	3	5	3	2	3	2	4		1	1	1				2	1	2	2	9	44	2	42
2	9	6	1	2	4	9	5	3	3	3	1	1	1		1		1		1	1	4	4	60	15.5	44.5
3	11	9	1		6	13	7	6	3		2	2	1		1	2				1	2	2	72	24	48
4	11	13	15	4		5	9	4	3	4	4	3	1	2	2	4	3	2	1	3	1	6	97	43	51
5	14	9	21	27	10		8	15	8	7	10	13	2	6	3	3	7	2	2	1	2	3	173	81	92
6	9	20	25	32	46	20	1	7	16	13	11	11	3	8	3	4	5	7	7	7	4	1	260	152.5	107.5
7	9	11	21	24	11	26	10	6	8	9	15	15	5	7	6	6	4	9	4	4	2	3	218	148	100
8	4	7	7	13	19	23	20	9		3	15	4	9	10	5	3	3	5	5	3	3	2	172	102	70
9		1	1	2	1	11	9	9	2	4	8	8	5		3	2	3	5		2	2	6	90	45.5	44.5
10		4	2	2	2	3	6	6	5	1		1	2	4	2	1	3		3	1	1		49	31	18
11		1			3	2	4		3	2	4	1		1	1	3			2	2	1	3	33	13.5	19.5
12												1			1	1				1	1		5	1	4
13				2								1			1			1					5	3	2
14																		1	1	1			3		3
15								1															1	1	
16											1			1	1								3	3	
17																	1				1		2	1	1
18						1						1						1					3	3	
19															1			1					2	2	
20	1													1								1	3	2	1
21																				1			1	1	
Total Sib's.	70	85	108	112	111	116	78	71	51	17	66	63	31	44	34	29	30	36	27	32	26	10	1,340	675	665
Older		1	8.5	6	16	30	31.5	13	11	10.5	64	59.5	31	42	32	29	29	34	27	31	26	10	665		
Younger	70	84	99.5	106	125	86	46.5	28	40	6.5	2	3.5		2	2		1	2		1			675		

TABLE 38c

MEASLES. AGE OF SIBLINGS IN A TWO-CHILD FAMILY WITH A SECONDARY CASE

Age of Primary Case	Age of Secondary Case																						Primary Cases		
	<1	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	Total	Older	Younger
<1			3		1																		4		4
1			2		2		1																5		5
2	4	5	1	2	3	5	1	2															23	9.5	13.5
3	3	8	3		2	6	3	2															27	14	13
4	5	10	15	1		1	3	3	2	1													41	31	13
5	5	6	18	21	10		5	7	6	1	2	1											85	63	22
6	5	17	20	26	35	18		2	5	1	2	2											136	121	15
7	2	11	19	19	33	21	7	5	2	1	1	4		1									132	114.5	17.5
8	1	1	7	14	17	22	15	8		1	4	1	2										93	85	8
9		3	2	2	4	11	8	8	2		5	3	1										49	40	9
10		1	1	1	1	2	3	5	5	2		1		1	1								24	21	3
11		1			2	2	1		3		1	1		2	1								14	10.5	3.5
12												1				1							2	1	1
13				2																			2	2	
14																	1			1			2		2
15								1															1	1	
16															1								1	1	
17																	1						1	1	
18																									
19															1								1	1	
20																									
21																									
Total Secondary	25	66	91	88	110	91	47	43	25	13	18	14	3	4	4	1	2			1			646	516.5	129.5
Older			5.5	2	8	15	13	18.5	15	11	17	12.5	3	4	2	1	1			1			129.5		
Younger	25	66	85.5	86	102	76	31	24.5	10	2	1	1.5			2		1						516.5		

TABLE 38*d*

MEASLES. AGE OF SIBLINGS IN A TWO-CHILD FAMILY WITH A SUSCEPTIBLE ESCAPE

Age of Primary Case	Age of Susceptible Escape																						Primary Cases		
	<1	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	Total	Older	Younger
<1		1	1																				2		2
1	2		2	1	3	2		2					1										13	2	11
2	5	1				2			1	1										1	1		12	6	6
3	8	1	1		2		1		1														14	10	4
4	6	3		3		1	3				1								1				18	12	6
5	9	3	3	3						1		1		1									21	18	3
6	4	2	5	6	7	2		1	4	1		2	1	2									37	26	11
7	7	4	2	5	8	3	1		3	1	3												37	30	7
8	3	1		1	2		1			1	2		1	1									13	8	5
9		1	2							1		2	1				1	1					9	3.5	5.5
10			1	1	1	1	2												1				7	6	1
11					1									1	1	1			1				5	1	4
12																									
13															1								1		1
14																									
15																									
16																									
17																									
18						1						1						1					3	3	
19																		1					1	1	
20	1																						1	1	
21																									
Total Susceptible Escape	15	17	17	20	24	12	8	3	9	6	6	6	4	5	2	1	1	3	3	1	1		194	127.5	66.5
Older		1	3	1	5	5	4	3	9	5.5	6	5	4	5	2	1	1	1	3	1	1		66.5		
Younger	15	16	14	19	19	7	4			0.5		1						2					127.5		

TABLE 38c

MEASLES. AGE OF SIBLINGS IN A TWO-CHILD FAMILY WITH AN IMMUNE

Age of Primary Case	Age of Immunes																						Primary Cases		
	<1	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	Total	Older	Younger
<1				1				2	1		1		1							2			8		8
1				2		1	1	1	2	1		1		1				2	1	2	2	9	26		26
2					1	2	1	1	2	2	1	1	1		1		1		1		3	4	25		25
3					2	7	3	1	2		2	2	1		1	2				1	2	2	31		31
4							3	1	1	3		3	1	2	2	4	2	2		3	1	6	34		31
5							3	8	2	5	8	11	2	5	3	3	7	2	2	1	2	3	67		67
6					1		1	1	7	8	9	7	2	6	3	4	5	7	7	7	4	1	86	4.5	81.5
7		1				2	2	1	3	4	8	11	1	6	6	6	4	9	1	3	2	3	79	5.5	73.5
8		1		1		1	1	1		1	9	3	7	9	5	3	3	5	5	3	3	2	66	8	58
9							1	1			3	3	3		3	2	2	1		3	2	6	33	2	31
10							1	1		2			2	3	1	1	3		2	1	1		18	4	14
11										2				1	2	2			1	2	1	3	11	2	12
12															1					1	1		3		3
13												1						1					2	1	1
14																	1	1					2		2
15																									
16											1			1									2	2	
17																					1		1		1
18																									
19																									
20														1								1	2	1	1
21																				1			1	1	
Total Immune		2		4	7	13	23	25	20	28	42	13	21	35	28	27	28	33	23	30	25	40	500	31	469
Older				3	3	10	14.5	21.5	20	21	41	12	21	33	28	27	28	33	23	29	25	10	469		
Younger		2		1	4	3	8.5	3.5		4	1	1		2						1			31		

5.37 and the age of single primaries is 6.77. It is noteworthy that the age of the older of the two primaries when there are two has an average value practically identical with that of the single primary when there is only one. The age distributions of the single primary and of the older of the double primaries are indeed much alike except at ages 0 and 1 where there naturally cannot be many "older" of double primaries though single primaries are by no means rare. The mean age of a single primary with a secondary case is 7.00, the age of the secondary averaging 4.91, signifying that those primaries which have a secondary case (not a second primary) are older than the general run of single primaries or than the older of the two in a double primary, whereas the secondary case is older than the younger of the double primaries by nearly a year. The average age of the primary where there is a susceptible escape is 6.44 and is close to the average of total primaries. In considering this figure we have to remember that many children under one who are ranked as susceptible escapes may really be immunes. The total number of children under 1 in this group is 45; if we eliminate all of these, the average age of the remaining primaries is 6.75, and somewhere between this figure and that of 6.44, perhaps around 6.60 would be the average age of the primaries with a true susceptible escape. Finally, the average age of a single primary with immune is 6.63 which would be somewhat reduced if the immunes under 1 were put in—although there is presumably little logic for such purposes as those of the present comparisons in classifying together those immunes who have carried over their immunity from their mother with those who have acquired it through contracting the disease.

39. *The Rôle of the Older and Younger Child.*—From Table 38*b* we see that among the single primary 2-child families the number of times the younger child is the primary case is 665, whereas the number of times the older child is the primary case is 675; these figures are essentially identical and show that it is "a matter of chance" whether it is the younger or the older child who introduces measles into the family. (The

families with a double primary case number 241 and if the cases be divided half and half as older and younger, we have the numbers 906 younger and 916 older.) These results include the 2-child families in which there is an immune. If we assume that the immune child and the adults (generally immune) do not appreciably bring measles into the family and if we therefore consider only families with two susceptibles we should find a different result. Of the two-susceptible 2-child families with a single primary case (summation of Tables 38*c* and 38*d*) there are 196 in which the younger child is primary and 644 in which the older is primary (whether we add to such numbers 241 cases to cover the double primary families or whether we allot them in the ratio of 644 : 192 is a matter of judgment), which shows that the susceptible child which brings measles into the two-child family is predominantly the older.

If now we turn to the question whether it is the older or the younger child which has measles, we have the 887 two-case families in which the assignment has to be half and half, and the 500 families with one immune in which the case is the younger of the children 469 times but older 31 times, and the 194 families with a susceptible escape in which the younger child is the case $66\frac{1}{2}$ times and the older is the case $127\frac{1}{2}$ times. If we eliminate the probable number (26) of susceptible escapes under one year of age and transfer them to the immunes, these numbers become 526 families with one immune with 469 cases among younger children, 57 among older children; 168 families with a susceptible escape in which the younger is the case $66\frac{1}{2}$ times and the older, $101\frac{1}{2}$ times. In any event the number of younger cases is $535\frac{1}{2}$ and of older ones $158\frac{1}{2}$ out of 694 families—which is a very unequal division and remains statistically unequal even if we add 887 to each figure to make $1422\frac{1}{2}$ younger and $1045\frac{1}{2}$ older children among the 2468 cases of measles in 2-child families.

One reason for entering so much into detail in the foregoing paragraph is to bring out the various possibilities of comparison with other infectious diseases such as scarlet fever

which have immunities acquired without frank clinical cases. Suppose, for example, that measles were a disease, such as some believe infantile paralysis to be, with a virus widespread over the population most of whom become immune from contact with it before reaching adult life, but with very few clinical cases. It would then be impossible to tell whether the case occurring in a family represented the primary infection of the family or was an infection secondary to some other and to classify the children as immunes or susceptibles on the basis of whether they had or had not had the disease in clinical form would be without immunological significance. Given a sufficient number of cases one might of course study multiple incidence within the family, but general statistical procedures such as are used for measles in discussing secondary attack rates would be of little use. The cases of such a disease would presumably be really comparable to "all cases" for measles and the division of 2-child families into those where the younger child was the case or the older was the case would presumably have to be compared with the division of all cases which for measles is unequal rather than of the primary cases which for measles is equal. If the virus were so common that most children developed their case or became immune sub-clinically during the first two years of life most of the cases in 2-child or any-child families would of necessity be among younger children, whereas if the virus were rare (even though very infectious in such close contacts as those within the family), the family would tend to be more nearly completed and the children to be older before the family became infected and there would tend to be relatively more older cases—provided, of course, that the chances of clinical manifestation of an infection did not decrease with age sufficiently to overcome this tendency. We shall have to bear such considerations in mind when we compare scarlet fever with measles, because scarlet fever seems somewhat intermediate in some respects between measles and the sort of disease infantile paralysis was supposed above to be, though perhaps having the further complication of existing in the carrier

condition more commonly than either measles or infantile paralysis.

40. *Summary of Statistical Constants.*—The statistical properties of the various tables are given in Table 39.

TABLE 39

MEASLES. STATISTICAL CONSTANTS FOR TABLES 38

Double Primaries (Table 38a)

	Total	Older	Younger
Freq.	482	241	241
Mean	5.37	6.74	4.00
S.D.	2.59	2.30	2.09
Q_1	3.45	5 24	2.38
Q_3	7.04	8.22	5.56
Med.	5.37	6.63	3.93
Mean Age Diff.*			0.00(2.74)
S.D. of Age Diff.*			3.18(1.63)
Correlation of Ages			0.32±.04(0.73±.03)

Primary-Secondary (Table 38c)

	Total Primary	Total Secondary	Primary Older	Primary Younger	Secondary Older	Secondary Younger
Freq.	646	646	516½	129½	129½	516½
Mean	7.00	4.91	7.27	5.90	8.41	4.15
S.D.	2.30	2.95	2.05	2.86	3.08	2.34
Q_1	5.69	2.78	6.10	3.80	6.18	2.45
Q_3	7.22	6.30	8.41	7.69	10.57	5.31
Med.	7.00	4.49	7.18	5.76	8.22	3.96
Mean Age Diff.*	2.08(3.09)					
S.D. of Age Diff.*	2.81(1.64)					
Correlation of Ages	0.45±.03					

Primary-Susceptible-Escape (Table 38d)

	Total Primary	Total Susceptible Escapes	Primary Older	Primary Younger	Susceptible Escapes Older	Susceptible Escapes Younger
Freq	194	194	127½	66½	66½	127½
Mean	6.44	5.16	6.83	5.70	9.80	2.74
S D.	3.26	4.79	3.19	3 26	4.44	2.75
Q_1	4.44	1.24	5.13	2.69	6.53	0.72
Q_3	7.78	8.00	7.74	8.08	12.72	4.11
Med.	6.47	3.92	6.62	6.16	9.50	2 23
Mean Age Diff.*	1.28(4.10)					
S.D. of Age Diff.*	4.85(2.89)					
Correlation of Ages	0.32±.06					

TABLE 39—*continued*

Primary-Immune (Table 38c)

	Total Primary	Total Immune	Primary Older	Primary Younger	Immune Older	Immune Younger
Freq.	500	500	31	469	469	31
Mean	6.64	13.70	10.0	6.41	14.11	7.4
S.D.	2.95	4.89	3.8	2.74	4.68	3.6
Q_1	5.02	10.08	7.7	4.82	10.53	5.4
Q_3	8.30	17.80	10.9	8.11	18.01	9.4
Med.	6.69	13.56	8.8	6.54	13.97	6.7
Mean Age Diff.*	−7.06(7.39)					
S.D. Age Diff.*	5.31(4.84)					
Correlation of Ages	0.16±.04					

* In these tables the values of age differences are algebraic in the first column and arithmetic in parentheses, the first column of the first table being derived by symmetrizing the table. In respect to the last two of the four tables it must be borne in mind that some of the children under one year of age were undoubtedly actually immune rather than susceptible escapes which would mean a transfer of perhaps 26 families from the third to the fourth table and result in a considerable change in some of the constants.

It will be observed that the correlation of the ages of the children in the two-child families decreases from the double primaries (asymmetrical table) through the families with two cases but only one primary and the families with a susceptible escape to the families with an immune, and that the arithmetical difference increases in the same order while the algebraic difference except for the first zero (fictitious) value decreases algebraically, and that the scatter in the age difference, whether algebraic or arithmetic, tends similarly to increase.

The regressions in Table 38*a* between the ages of double primaries seem essentially linear in view of the small number of data and the actual fluctuations of the individual points which, though larger than their chance standard deviations might well permit, are apparently irregular rather than systematic. The regression equations of the age of the older or of the younger of the double primaries on that of the other are:

$$(\text{Young} - 4.00) = 0.661\ (\text{Old} - 6.74)$$
$$\text{or Young} = 0.661\ \text{Old} - 0.46$$
$$(\text{Old} - 6.74) = 0.807\ (\text{Young} - 4.00)$$
$$\text{or Old} = 0.807\ \text{Young} + 3.51$$

41. *Secondary Attack Rate by Age Difference.*—There is some interest in the secondary attack rate in families with one primary case and one susceptible when arranged by the age difference primary − secondary. The table gives the result.

Age Difference	Susceptibles	Secondary Attack Rates	Age Difference	Susceptibles	Secondary Attack Rates
20 to 9 years	6	67	−1	38	68
8	11	55	−2	56	73
7	24	46	−3	44	73
6	37	76	−4	21	76
5	75	80	−5	13	69
4	104	78	−6	8	25
3	156	82	−7 to −18	12	0
2	157	87	Mean	192	65.6
1	70	84	0	8	87.5
Mean	640	80.2	Total	840	76.9

Thus the secondary attack rate is low when the age difference is great whether this be positive or negative and the rate is reasonably constant from + 6 to + 1 years of age difference at around 81 whereas it is also reasonably constant from − 1 to − 5 years at around 72 (the value for twins of which there were 7 pairs was 87.5). The difference between 81 and 72, or the still greater difference in the means 80.2 and 65.6, is statistically significant. There is, however, such a difference in age distribution between the cases above and below zero age difference that an adjustment therefor brings the figure 80.2 down to 76.2 and raises the figure 65.6 to 72.8. The difference of 3.4 per cent in the age adjusted secondary attack rates for the two groups in which the primary case is older or younger respectively than the sibling is not statistically significant being only about equal to the standard deviation of that difference.[21]

[21] The application of the adjustment for age though straightforward arithmetically may be called in question on logical grounds. We have evidence that the secondary attack rate depends (1) on the age of the susceptible and (2) on the algebraic difference of age between the primary case and the sibling. It is probable that the secondary attack rate is actually a function of the two ages of primary and of susceptible sibling. As, however, we have not a large enough number of cases to discuss the secondary attack rate as a function of the two variables, we can only treat the matter in a somewhat illogical fashion.

The very low secondary attack rates when the age difference is great and negative, there being no case among 12 susceptibles with age difference − 7 to − 18, taken with the fact that the secondary attack rates above 14 years of age without regard to the age of the primary case are also small, suggest rather forcibly that there must be a considerable degree of real immunity at the older ages. Whether this immunity is due to having had subclinical cases or to having forgotten that one had had a clinical case or partly to one and partly to the other cause, or possible to still other causes,[17] we cannot say. The matter deserves very careful study, difficult as that study would presumably be. Stocks and Karn [3] give some evidence that susceptibles who are exposed to measles within the family in one epidemic and do not develop a clinical case acquire a temporary immunity lasting perhaps two years (and on this evidence they base a theory of the two-year periodicity of measles). It may be suggested that possibly susceptibles who are several times exposed to measles without developing a case may gradually acquire sufficient immunity so that they would never develop a frank case on exposure. It is believed that in some instances young children still having some degree of immunity from their mother, when exposed within the family, may have a case so mild or modified as to escape notice and yet sufficient to give a life long immunity.

Scarlet Fever in the Two-Child Family

42. *Cases, Immunes and Susceptibles.*—As there seems to be no definite period which can be taken to cover double primaries, Table 40 analogous to Table 37 for measles will contain the entries, primaries, secondaries, immunes and susceptible escapes.

The mean age of the primaries is 7.48, of secondaries is 5.56, of immunes 11.13, and of susceptible escapes 7.92. The mean age of the primaries is about 8½ months greater than for single primaries in measles and 13 months greater than for all primaries. The mean age of secondaries for scarlet fever

TABLE 40

SCARLET FEVER. AGE DISTRIBUTION OF PRIMARIES, SECONDARIES, IMMUNES, AND SUSCEPTIBLE ESCAPES IN THE TWO-CHILD FAMILY

Age	Primary	Secondary	Immune	Susceptible Escape
<1	4	1		33
1	19	7		43
2	34	14	1	39
3	46	14	1	41
4	58	16	2	52
5	76	8	4	28
6	111	9	5	34
7	91	10	1	41
8	67	6	7	28
9	49	4	2	26
10	37	1	8	30
11	19	2	3	22
12–14	61	4	12	53
15–21	22	1	11	70
Total	694	97	57	540

is about 8 months greater than for measles. The mean age of the immunes is about $2\frac{1}{2}$ years less and of susceptible escapes about $2\frac{3}{4}$ years greater than for measles in the two-child family; we have however to realize that neither of these two classes has the same immunological significance as in measles.

43. *Various Age Correlations.*—Table 41*a* gives the correlation of age of primary against age of sibling, Table 41*b* that of age of primary against age of secondary, Table 41*c* that of age of primary against age of susceptible escape, and Table 41*d* that of age of primary against age of immune. The first table is thus simply the sum of the last three.

44. *Comparison of Mean Ages of Primaries and Rôle of Older and Younger Child.*—The mean age of the primary with a secondary is 6.89 whereas that of all single primaries is 7.48, the age of the secondary being 5.56. In measles the mean age of a (single) primary with a secondary case was 7.00 whereas that of all single primaries was 6.77 and of all primaries was 6.40. Thus the mean age of a primary with a secondary case in scarlet fever is less than that of all primaries whereas in measles it is greater. In scarlet fever the mean

TABLE 41*a*

SCARLET FEVER. TWO-CHILD FAMILY WITH ONE PRIMARY CASE, AGES OF SIBLINGS

Age of Primary Case	Age of Sibling																						Primary Cases		
	<1	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	Total	Older	Younger
<1		1			1	1				1													1		4
1	3	1	1	2	1	2	3	1	2							1			2				19	3.5	15.5
2	6	4		5	9	1	6	2											1				31	10	24
3	2	12	6		5	2	5	2	5		2		2	1			1					1	46	20	26
4	6	8	12	4		4	7	7	4	1								2	1		2		58	30	28
5	3	7	9	11	6	1	5	8	5	2	3	3	4	3	2		2				2		76	36.5	39.5
6	6	6	17	15	19	7	2	4	8	4	3	4	2	2	2	1	3	3	1		2		111	71	40
7	1	3	3	8	11	10	4	1	6	12	8	4	9	2	5			2		1	1		91	40.5	50.5
8	3	4	3	5	8	5	5	8	1	2	8	6	1	1	2		2		1	1	1		67	41.5	25.5
9		2		3	4	3	5	7	2		5	4	3	1	1	1	2	2		2	1	1	49	26	23
10	1	1		3	3	2	1	6	2	3	1		4	2	3	2			1	1	1		37	22.5	11.5
11						2	1	3	1	4	2			1	1	1		1	1	1			19	13	6
12	1		1		1		3	2	3	2	3			2	2	3	3	3	2				31	16	15
13	2				2		1		1		2	4	1		1				3	1	1		19	13	6
14			1									2	2	2				1	1		1	1	11	7	4
15										1			1	2				2	1				7	4	3
16									1						1				1		1		1	2	2
17														1							2		3	1	2
18											1									1			2	1	1
19			1																			1	2	1	1
20								1			1												2	2	
21		1																	1				2	2	
Total Sib's.	34	50	54	56	70	40	18	52	41	32	39	27	29	20	20	9	13	16	17	8	15	4	694	363.5	330.5
Older		1.5	1	7	16	10.5	27	24.5	30.5	22	29.5	21	25	15	19	9	13	16	16	8	15	4	330.5		
Younger	34	48.5	53	49	54	29.5	21	27.5	10.5	10	9.5	6	4	5	1				1				363.5		

TABLE 41b

SCARLET FEVER. AGE OF SIBLINGS IN A TWO-CHILD FAMILY WITH A SECONDARY CASE

Age of Primary Case	Age of Secondary Case																						Primary Cases		
	<1	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	Total	Older	Younger
<1						1																	1		1
1				1		1																	2		2
2				3			2																5		5
3		3	1		1				1														6	4	2
4		2	2	1		1	4		1	1													12	5	7
5		1	2	2	3		1	2															11	8	3
6	1	1	4	3	3	1	1	2	2														18	13.5	4.5
7			2	2	4	2			1	2			1										14	10	4
8			1	1	1			3				1		1									8	6	2
9					1	1		2			1	1											6	1	2
10				1	1		1							1									4	3	1
11						1		1															2	2	
12			1		1				1	1													4	4	
13					1								1								1		3	2	1
14			1																				1	1	
Total Secondary	1	7	14	14	16	8	9	10	6	4	1	2	2	2							1		97	62.5	34.5
Older				4	1	3	7.5	4	5	3	1	2	1	2							1		34.5		
Younger	1	7	14	10	15	5	1.5	6	1	1			1										62.5		

TABLE 41c

SCARLET FEVER. AGE OF SIBLINGS IN A TWO-CHILD FAMILY WITH A SUSCEPTIBLE ESCAPE

Age of Primary Case	Age of Susceptible Escape																						Primary Cases		
	<1	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	Total	Older	Younger
<1		1			1					1													3		3
1	3	1	1	1	1	1	3	1	1							1			2				16	3.5	12.5
2	6	4		2	9	1	4	2															28	10	18
3	2	9	5		4	2	5	2	4				2				1					1	37	16	21
4	6	6	9	3		2	2	6	1									2	1		2		40	21	16
5	3	6	7	9	3	1	2	6	3	2	2	2	2	3	2		2				2		57	28.5	28.5
6	5	5	13	12	16	6	1	2	5	1	3	3	1	2	1	1	3	3	1		2		89	57.5	31.5
7	1	3	1	6	6	7	1	1	4	9	6	1	6	1	1			1		1			65	28.5	36.5
8	3	4	2	4	6	4	4	5	1	2	7	4	1		2		2		1	1	1		54	32.5	21.5
9		2		3	3	2	4	5	2		1	3	3	1	1	1	2	1		2	1	1	41	21	20
10	1	1		1	2	1		6	2	2			1	1	3	2			1	1			28	16	12
11						1	1	2	1	4	2			1	1			1	1	1			16	11	5
12	1						3	2	2	1	2			2	2	3	2	3	2				25	11	11
13	2				1		1		1		2	4			1				2	1			15	11	4
14												2		1					1		1		5	3	2
15										1			1	2				2	1				7	4	3
16									1						1				1		1		1	2	2
17														1							2		3	1	2
18											1												1	1	
19			1																			1	2	1	1
20								1			1												2	2	
21		1																	1				2	2	
Total Susceptible Escape	33	43	39	41	52	28	34	41	28	26	30	22	20	15	18	8	12	13	15	7	12	3	540	286.5	253.5
Older		1.5	1	3	15	6.5	16.5	19.5	18.5	18	22	16	19	11	17	8	12	13	14	7	12	3	253.5		
Younger	33	41.5	38	38	37	21.5	17.5	21.5	9.5	8	8	6	1	4	1				1				286.5		

TABLE 11d

SCARLET FEVER. AGE OF SIBLINGS IN A TWO-CHILD FAMILY WITH AN IMMUNE

Age of Primary Case	Age of Immune																						Primary Cases		
	<1	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	Total	Older	Younger
<1																									
1									1														1		1
2																			1				1		1
3											2			1									3		3
4			1			1	1	1	2														6	1	5
5							2		2		1	1	2										8		8
6									1			1	1		1								1		4
7					1	1			1	1	2		2	1	1			1			1		12	2	10
8					1	1	1				1	1											5	3	2
9							1											1					2	1	1
10				1		1				1	1										1		5	3.5	1.5
11																1							1		1
12											1						1						2	1	1
13																			1				1		1
14													2	1				1				1	5	3	2
15																									
16																									
17																									
18																				1			1		1
Total Immune			1	1	2	4	5	1	7	2	8	3	7	3	2	1	1	3	2	1	2	1	57	14.5	42.5
Older						1	3	1	7	1	6.5	3	5	2	2	1	1	3	2	1	2	1	42.5		
Younger			1	1	2	3	2			1	1.5		2	1									14.5		

age of those primaries which have an immune is 8.01 (as compared with 7.48 for single primaries), in measles 6.64 (as compared with 6.77 or 6.40). In scarlet fever the mean age of these primaries which have a susceptible escape is 7.53 (practically 7.48) and in measles 6.44.

The number of times the younger child is the primary case is 330½ and the older 363½. These numbers are not so near together as for measles but their difference 33 is only slightly greater than its standard error. Hence in scarlet fever as in measles one has statistically practically an even division between older and younger children in respect to being a primary case. In measles for the 2-susceptible 2-child family with a single primary we found 196 families in which the younger child was the case and 644 families in which the older child was the case. For scarlet fever the younger child of two susceptibles is the case in 288 families and the older in 349; the disparity being in the same direction but not nearly so strong as in measles. If we consider all cases of scarlet fever, the younger child has 393 and the older 398 and hence the cases are evenly distributed among the younger and the older of the two children instead of showing a great preponderance of younger children as in measles. Among the secondary cases the younger in scarlet fever has about twice as many as the older whereas in measles it was about four times as many.[22]

[22] Although the determination of sex, being dependent on the interpretation of given names, is not reliable so that we have rarely discussed matters on the basis of sex we may offer some four-fold tables showing for the 2-child family the separation by sex (no allowance for immunity under one year of age).

For Measles

Double Primaries

Older	Younger M	Younger F	Total
M	65	54	119
F	59	63	122
Total	124	117	241

Primary-Secondary

Primary	Secondary M	Secondary F	Total
M	163	175	338
F	160	148	308
Total	323	323	646

45. *Summary of Statistical Constants.*—The statistical constants for the different tables 41 are summarized in Table 42.

It may be observed that: The correlation of ages is essentially the same (0.40) for all three tables whereas it decreased

FOR MEASLES—*continued*

Primary-Susc. Esc.

Primary	Susc. Esc. M	Susc. Esc. F	Total
M	40	57	97
F	61	36	97
Total	101	93	194

Primary-Immune

Primary	Immune M	Immune F	Total
M	112	136	248
F	120	132	252
Total	232	268	500

There is association in the double primary table, like sexes occurring more frequently, but it is not statistically significant In the primary-secondary table there is dissociation, unlike sexes occurring more frequently, but it is not significant. In the table for primaries and susceptible-escapes there is dissociation, unlike sexes occurring more frequently and it is significant. In the primary-immune table there is dissociation but it is not significant.

FOR SCARLET FEVER

Primary-Secondary

Primary	Secondary M	Secondary F	Total
M	23	28	51
F	21	25	46
Total	44	53	97

Primary-Susc. Esc.

Primary	Susc. Esc. M	Susc. Esc. F	Total
M	131	139	270
F	145	125	270
Total	276	264	540

Primary-Immune

Primary	Immune M	Immune F	Total
M	13	14	27
F	18	12	30
Total	31	26	57

There is dissociation here in all three of the tables as there was in the corresponding tables for measles; none of the dissociations is, however, significant.

TABLE 42

SCARLET FEVER. STATISTICAL CONSTANTS FOR TABLES 41

Primary-Secondary (Table 41b)

	Total Primary	Total Secondary	Primary Older	Primary Younger	Secondary Older	Secondary Younger
Freq...	97	97	62½	34½	34½	62½
Mean	6.89	5.56	7.65	5.51	8.07	4.18
S.D.	2.93	3.29	2.70	2.83	3.43	2.22
Q_1	4.90	3.20	5.89	3.56	6.15	2.58
Q_3	8.53	7.42	9.34	7.47	9.62	5.08
Med.	6.67	4.81	7.12	5.25	7.56	3.98
Mean Age Diff.*	1.33(3.14)					
S.D. of Age Diff.*	3.54(2.10)					
Correlation of Ages	0.36±0.09					

Primary-Susceptible-Escape (Table 41c)

	Total Primary	Total Susceptible Escapes	Primary Older	Primary Younger	Susceptible Escapes Older	Susceptible Escapes Younger
Freq...	540	540	286½	253½	253½	286½
Mean..	7.53	7.92	7.93	7.08	11.85	4.45
S.D.	3.61	5.48	3.61	3.56	4.81	3.23
Q_1	5.20	3.50	5.65	4.59	8.05	1.94
Q_3	9.40	11.48	9.71	9.11	15.77	6.36
Med...	7.01	7.01	7.15	6.90	11.36	3.82
Mean Age Diff.*	−0.39(4.09)					
S.D. of Age Diff.*	5.17(3.19)					
Correlation of Ages	0.41±0.04					

Primary-Immune (Table 41d)

	Total Primary	Total Immune	Primary Older	Primary Younger	Immune Older	Immune Younger
Freq....	57	57	14½	42½	42½	14½
Mean Age	8.01	11.13	10.2	7.28	12.38	7.5
S.D.	3.53	4.61	2.8	3.44	4.26	3.5
Q_1	5.47	8.11	8.4	5.14	8.88	5.0
Q_3	10.25	13.75	12.9	8.19	15.88	10.9
Med.	7.50	10.75	10.2	6.94	11.75	6.4
Mean Age Diff.*	−3.12(4.49)					
S.D. of Age Diff.*	4.52(3.16)					
Correlation of Ages	0.41±0.11					

* In these tables as in those (Table 39) for measles the values of the age differences are algebraic in the first column and arithmetic in parenthesis.

in the order of these tables (Table 39) for measles from 0.45 to 0.16. The arithmetical difference of the ages increases through the three tables whereas the algebraic difference decreases, as for measles. The mean arithmetical age difference of primary and secondary and of primary and susceptible escape are respectively the same for scarlet fever and measles, but this difference for primary and immune is much larger for measles than for scarlet fever. The algebraic mean age differences are numerically less in all groups for scarlet fever than for measles.

46. *Secondary Attack Rate by Age Difference.*—The number of secondaries is so small that coarser grouping than for measles is necessary. The age difference is primary − secondary, and the secondary attack rates are:

Age Difference	Susceptibles	Secondary Attack Rates	Age Difference	Susceptibles	Secondary Attack Rates
20 to 9 yrs.. ..	13	23	−1 to −4 yrs.	178	16
8 to 5 .	66	17	−5 to −8	68	9
4 to 1 . .	267	18	−9 to −18	39	0
Mean .. .	346	17.9	Mean	285	11.9
Adjusted Mean		13.5	Adjusted Mean		13.8

(The secondary attack rate for 0 age difference is 17 based on one secondary.) The numbers are too small to place much dependence on the individual items, but on the whole there is here seen a drop of secondary attack rate from cases when the primary is older to those in which the primary is younger. The difference between the means is 6.0 with a standard deviation of 2.8 which would mean significance. On the other hand the secondary attack rates for scarlet fever vary with age as they do for measles and such a difference should be discussed on the basis of some age adjustment of the means. The difference in the adjusted means is 0.3 and if we assume the same standard deviation 2.8 for this difference, the difference is not significant.

GREENWOOD'S "BRAT"[23] AND OTHER MATTERS IN FAMILIES OF MORE THAN TWO SUSCEPTIBLES

47. *Statement of the Theory.*—Greenwood has proposed a theory, the chain binomial, to account for the number of cases observed in families with 2 or more susceptibles in addition to the primary case. Briefly the theory is this: There is a short period of infectiousness during which the chance of passing the infection to a susceptible is p. Thus in N families with a single primary case and m susceptibles there will be an average of Nmp immediate secondary cases and Nmq, where $q = 1 - p$, escapes from the infectious primary. However for the N families there will be probabilities of p^m, $mp^{m-1}q, \cdots, q^m$ for an actual number of immediate secondaries equal to $m, m - 1, \cdots, 0$. In case there is one or more, but not m, such secondaries, they may in their brief turn infect some of the remaining $m - 1, \cdots, 1$ susceptibles thus producing a second generation of secondaries which may be called tertiary cases, and so on. The types of chain which exist for a family with $m = 2$ susceptibles are as follows: (1) Two immediate secondaries, (2) One immediate secondary and one tertiary case, (3) One secondary case and one escape, and (4) Two escapes; furthermore, the probabilities of these four possibilities under the theory are respectively p^2, $2p^2q$, $2pq^2$, q^2, of which the sum is necessarily 1. The mean number of secondary cases of all orders per family is $2 - 4q^2 + 2q^3$ and, there being 2 children per family, the secondary attack rate (per capita, not per cent) is $r = 1 - 2q^2 + q^3$ which must be distinguished from the chance p of a case giving rise to another immediate case. In fact the value of q may be obtained by setting $1 - 2q^2 + q^3$ equal to the secondary attack rate. Greenwood tests his theory by comparing the observed number of families with 2, 1, 0 secondary cases (altogether) with the numbers $Np^2(1 + 2q)$, $2Npq^2$, Nq^2 predicted by the theory; he appears not to have attempted to check the theory

[23] See reference 5. In the final paragraph (p. 351) Greenwood thus depreciatingly refers to his theory. Greenwood takes over his theory into his book, *Epidemics and Crowd Diseases*, 1935, pp. 186–187.

by comparing the numbers Np^2, $2Np^2q$, $2Npq^2$, Nq^2 with the number of families of each of the four types which his theory postulates. Thus in his example, limited to families with two susceptibles under 10 years of age, in addition to the primary case, he has $N = 299$, $p = 0.360$, $q = 0.640$, the secondary attack rate is 44.3 per cent, and the number of families with two cases altogether should be 88.4 divided into two types, viz., 38.8 families with two secondaries immediately due to the primary case and 49.6 families with one secondary and one tertiary case. Greenwood found 86 families with two cases but did not give the enumerations separately of the two types of families to determine whether they were near to 39 and 50 respectively. Owing to the irregularity and delay with which some cases come down it is indeed difficult in many families to assign with any confidence the type of family whether double secondary or secondary-tertiary.

Greenwood applies his theory to cases in which there are two bona fide susceptibles and also to cases in which there are known immunes in the family of two children (under ten) in addition to the primary case, although it is difficult to see what is the logic of applying a refined theory of mode of passing an infection within the family to families where the passage of the infection is somewhat limited because of acquired immunity. Thus in another example there were 358 families with two children under 10 years of age. For the 358 families $p = 0.256$, $q = 0.744$, the secondary attack rate is 30.4 per cent (though we should not compute a secondary attack rate with known immunes remaining in the data) and the 58.3 families which his theory gives as having two cases is certainly a good fit to the observed number of 57 but is not subdivided into the two types, namely 23 with two immediate secondaries and 35 with the secondary-tertiary combination. The fact that the theory fits so well, as he fits it, by combining certain types of family is of very little value in establishing the theory unless it does indeed fit in detail as propounded; and the very fact that the theory fits equally well

when known immunes are left in as when they are omitted would of itself cast doubt on the validity of the theory—for whoever heard that a close fit of a theory to cases in which it was logically inapplicable was good evidence of the validity of the theory?

We may examine the theory on our Providence records. Before doing so it should be observed that the St. Pancras data of Stocks for the 1926 epidemic, which were used by Greenwood, may be better than the Providence material. Stocks and Karn [3] claim to find something like 70 per cent of measles reported whereas we have estimated reporting as at about 47 per cent (Hedrich [24] gives 32 per cent for Baltimore). Now, obviously, it is not to be assumed that an epidemiological sample of 47 per cent is as good as one of 70 per cent, and particularly as it cannot be assumed that in either sample the unreported cases (30 per cent or 53 per cent) are epidemiologically similar to the reported cases.[25] Moreover, Greenwood's data taken from Stocks show secondary attack rates of 25 to 35 per cent with all children under 10 years of age left in whether immune or not and of 45 to 50 per cent with the immunes (those who had had measles) rejected, whereas we find in Providence secondary attack rates on susceptibles under 10 running 75 to 80 per cent, and on all children under 10 from 60 to 65 per cent. Apparently, then, measles is far

[24] A. W. Hedrich, "Monthly Estimates of the Child Population 'Susceptible' to Measles, 1900–1931, Baltimore, Md." *Am. Jour. Hyg.*, 17, 613–636, 1933.

[25] This difficulty of the possibility, and indeed the probability, that the reported cases are not a fair sample of all cases has been referred to before. In what respects the reported cases are and in what respects they are not a fair sample is of course unknown, but just as "rates" have given different pictures, and in some respects decidedly different pictures, from those given by percentage distributions, so we should suspect that a true sample would give a different picture and perhaps in some respects a very different picture of the statistical-epidemiological relationships than the sample we actually have in the reported cases. Indeed we are inclined to believe that one of the most important advances which could be made would inhere in a determination of what sorts of cases fail of reporting and why. Moreover this is a matter of importance to the public health administrator quite as much as to the epidemiologist who works with the administrator's reported figures. And, finally, it is a matter on which the administrator can by special studies throw considerable light as Dr. Chapin in the conduct of his office did throw light on so many other matters of epidemiological and administrative importance.

less infectious under conditions of family life in St. Pancras than in Providence. One need not overlook the possibility that with only 47 per cent reported in Providence, it may be that those families with a large number of secondary cases in proportion to their total susceptibles are disproportionately frequent in the reported as compared with the unreported families. The discrepancy however between the secondary attack rate in St. Pancras and Providence is so great that the rates cannot well be reconciled on the hypothesis that there is a differential in favor of higher attack rates in the reported families in Providence unless one also assumes a differential in favor of lower attack rates in the reported as compared with the unreported families in St. Pancras. This leaves a quite enigmatic situation and makes any comparisons problematical.[26]

48. *Families with Two Susceptibles.*—Let us take first the simple case of the measles family of three children or of three susceptibles of whom one is the primary case (multiple primaries being excluded). For this type of situation Greenwood has two sets of data, already alluded to, which we list as A and B.

[26] We have computed the morbidity rates by age (cases divided by population) from the data published by Stocks and Karn,[3] pp. 367–8) for cases of measles in St. Pancras 1922–4 and populations at mid-year 1923. The morbidity rate per thousand for the three year period which contained two crests and one trough of measles are given with the comparative figures for Providence based on six years which contained one major epidemic (6/7 of all cases) and a minor one.

Age	<1	1	2	3	4	5	6	7	8	9
St. Pancras . . .	135	247	238	278	536	577	284	72	38	22
Providence	113	214	226	248	293	339	361	339	248	124

It will be seen that the rates are very different by age, but such differences might not be due so much to reporting as to the previous history of measles in the two places, to density of population, etc However, if the age distribution of cases unreported differs greatly from that of cases reported and if such differences are dissimilar in the two places there might be considerable "correction" to be made to the rates in one place or the other to get to a fair basis of comparison. We have no way to check up on the differences in the secondary attack rates in St. Pancras and in Providence but have to leave it as a problem.

A: $m = 2$. Families with one primary case and with $m = 2$ susceptibles and immunes both under 10 years of age. Frequencies of 0, 1, 2 subsequent cases with fitted values in parentheses and values of q, of p, of the probability P of a worse fit, of the false secondary attack rate r (per capita not per cent), and of the computed values for different types of family history.

	Families obs. calc	Cases		calc.
0	197 (198.3)	0	$q=0.744$	$Nq^2=198.3$
1	104 (101.4)	104	$p=0.256$	$2Npq^2=101.4$
2	57 (58.3)	114	$r=0.304$	$\begin{cases} 2Np^2q = 34.9 \\ Np^2 = 23.4 \end{cases}$
Total	358	218	$P=0.75$	

B: $m = 2$. Families with one primary case and $m = 2$ susceptibles both under 10 years of age. Frequencies of 0, 1, 2 subsequent cases with fitted values in parentheses and values of q, of p, of the probability P of a worse fit, of the secondary attack rate r (per capita), and of the computed values for the different types of family history.

	Families obs. calc	Cases		calc
0	120 (122.4)	0	$q=0.640$	$Nq^2=122.4$
1	93 (88.2)	93	$p=0.360$	$2Npq^2= 88.2$
2	86 (88.4)	172	$r=0.443$	$\begin{cases} 2Np^2q = 49.6 \\ Np^2 = 38.8 \end{cases}$
Total	299	265	$P=0.56$	

We shall now tabulate our own data giving in parentheses after the values in the last column the best estimates we can make from the intervals in days between cases of the numbers of families of the different types.[27]

[27] Note particularly that the rôle of the parenthesis is different. In the count of total families of the different types the observed frequencies are without parentheses and the calculated frequencies, given to tenths, are in parentheses, but in the distribution over subtypes the calculated values, given to tenths are not in parentheses whereas the observed values, observed as well as we can from the intervals between cases, are in parentheses.

I: $m = 2$. Three-child families with one primary case and $m = 2$ susceptibles of all ages (under 22) counting all children under 1 year as susceptibles.

	Families obs. (calc)	Cases		calc. (obs)
0	51 (51.2)	0	$q=0.351$	$Nq^2 =$ 51.2 (51)
1	67 (66.5)	67	$p=0.649$	$2Npq^2 =$ 66.5 (67)
2	298 (298.3)	596	$r=0.797$	$2Np^2q =$ 123.0 (36) $Np^2 =$ 175.3 (262)
Total	416	663	$P=0.94$	

The fit to the collected families is very good but the distribution of the families with 2 subsequent cases is totally at variance with the figures predicted by the theory; the number of double immediate secondaries is far in excess of the prediction and the number of secondary-tertiary instances is far below that of the theory, showing that the chain theory (and chain binomial) appear not to work in this case.

II: $m = 2$. Three-child families with one primary case and $m = 2$ susceptibles under 10 years and over 7 months of age (unless the child under 7 months was actually a case).

	Families obs. (calc.)	Cases		calc. (obs)
0	34 (27.1)	0	$q=$ 0.285	$Nq^2 =$ 27.1 (34)
1	25 (38.8)	25	$p=$ 0.715	$2Npq^2 =$ 38.8 (25)
2	275 (268.1)	550	$r=$ 0.861	$2Np^2q =$ 97.3 (36) $Np^2 =$ 170.8 (239)
Total	334	575	$P= <0.01$	

Here, although we are confining the children really to being presumably susceptible so far as we may, we find a bad fit to the collected families and, as before, an impossible one to the chains, the number of secondary-tertiary cases being far rarer than predicted.

III: $m = 2$. Families with more than 3 children and with one primary case and $m = 2$ susceptibles of all ages counting all children under 1 year as susceptibles.

	Families obs.	calc	Cases		calc.	obs
0	28	(31.2)	0	$q=0.364$	$Nq^2=31.2$	(28)
1	46	(39.6)	46	$p=0.636$	$2Npq^2=39.6$	(46)
2	161	(164.2)	322	$r=0.783$	$2Np^2q=69.2$	(27)
					$Np^2=95.0$	(134)
Total	235		368	$P=0.22$		

The fit is satisfactory to the collected families but again bad when the families of two cases are distributed.

IV: $m = 2$. Families with more than three children and with one primary case and $m = 2$ susceptibles under 10 years and over 7 months (except as the infant developed measles).

	Families obs	calc	Cases		calc	obs
0	15	(14.2)	0	$q=0.277$	$Nq^2=14.2$	(15)
1	19	(20.6)	19	$p=0.723$	$2Npq^2=20.6$	(19)
2	151	(150.2)	302	$r=0.868$	$2Np^2q=53.6$	(22)
					$Np^2=96.6$	(129)
Total	185		321	$P=0.68$		

The fit is good except that the chain theory seems to fail verification as before.

V: $m = 2$. Summation of I and III giving all families (of 3 or more children) with one primary case and 2 susceptibles of all ages counting all under 1 year as susceptible.

	Families obs	calc	Cases		calc.	obs
0	79	(82.4)	0	$q=0.356$	$Nq^2=82.4$	(79)
1	113	(106.2)	113	$p=0.644$	$2Npq^2=106.2$	(113)
2	459	(462.4)	918	$r=0.792$	$2Np^2q=192.2$	(63)
					$Np^2=270.2$	(396)
Total	651		1031	$P=0.46$		

The fit is again good as tested by Greenwood but worthless if the "chain" contribution of the two case families is examined.

VI: $m = 2$. Summation of II and IV giving all families of three or more children with one primary case and 2 susceptibles under 10 years and over 7 months (unless a case).

	Families obs.	Families calc	Cases			calc	obs.
0	49	(41.3)	0	$q=0.282$	$Nq^2=$	41.3	(49)
1	44	(59.3)	44	$p=0.718$	$2Npq^2=$	59.3	(44)
2	426	(418.4)	852	$r=0.863$	$2Np^2q=$	150.9	(58)
					$Np^2=$	267.5	(368)
Total	519		896	$P=0.02$			

The fit is not good to the collected families and quite impossible in respect to chains.

VII: $m = 2$. For scarlet fever. Three children under 10 years none of whom has had a case of scarlet fever whether in a 3-child family or in a larger family provided there are no susceptibles 10 years old or over. (For this illustration the primary case was itself under 10 years of age which was not always true in the six examples from our measles data.)

	Families obs.	Families calc.	Cases	
0	172	(166.7)	0	$q=0.842$
1	42	(52.6)	42	$p=0.158$
2	21	(15.7)	42	$r=0.179$
Total	235		84	$P=0.04$

In this case we have no satisfactory way of splitting the 21 families with two cases subsequent to the primary into two groups to determine the number of double immediate secondaries and the number of secondary-tertiary instances. The fit to the collected families is not satisfactory. It is unfortunate that we have not figures for scarlet fever that would give a chance to make more fits to the collected data even if we could not break down the count further to test the chain theory. It is possible, however, that Greenwood might not consider the chain-theory applicable because of a longer period of infectiousness and a carrier condition for scarlet

fever—one of his postulates being a very short period of infectiveness.

49. *Families with Three Susceptibles.*—Greenwood gives further cases in which $m = 3$ which we need not tabulate, but shall proceed at once to give our own results for $m = 3$.

VIII: $m = 3$. Four-child measles family with one primary case and 3 susceptibles of all ages (counting those under 7 months as susceptibles).

	Families obs.	Families calc.	Cases			calc.	obs.
0	10	(9.4)	0	$q=0.392$	$Nq^3=$	9.4	(10)
1	7	(6.5)	7	$p=0.608$	$3Npq^4=$	6.5	(7)
2	39	(33.1)	78	$r=0.817$	$6Np^2q^4=$	7.9	(3)
					$3Np^2q^2=$	25.2	(36)
3	95	(101.0)	285	$P=0.48$	$6Np^3q^3=$	12.7	(4)
					$3Np^3q^2=$	15.6	(3)
					$3Np^3q=$	39.8	(13)
					$Np^3=$	33.9	(75)
Total	151		370				

Here again we find a good fit to the number of families of total specified number of cases when no attention is paid to the way the cases arise under the chain theory. For 2 cases we have two possibilities—a direct secondary followed by a tertiary case secondary to it (chain) or a pair of secondary cases deriving directly from the primary case. It will be noted that there is according to the count an excess of direct double secondaries and a deficiency of secondary-tertiaries. When there are three cases the triple secondaries (Np^3) deriving directly from the primary are much more numerous than they should be by theory, whereas there is a deficiency of count below theory for all types of chaining, namely, the direct double secondary followed by a tertiary case deriving from one of the secondaries ($3Np^3q$), the single direct secondary followed by a double tertiary deriving from it ($3Np^3q^2$), and the full chain of secondary-tertiary-quaternary deriving in sequence from the primary ($6Np^3q^3$).

Case VIII for $m = 3$ is analogous to Case I for $m = 2$ as regards the conditions imposed on the families. We have the

analogs of cases II to VI inclusive which we shall number IX to XIII respectively and tabulate suppressing the "cases" which are easily calculated from the frequencies of the families.

	IX	X	XI	XII	XIII
0	4	10	4	20	8
1	3	15	6	22	9
2	9	29	7	68	16
3	84	44	38	139	122
N	100	98	55	249	155
q	0.299	0.517	0.417	0.457	0.347
p	0.701	0.483	0.583	0.543	0.653
r	0.910	0.697	0.812	0.770	0.875
P	0.04	0.18	0.04	0.40	<0.01
	calc obs	calc obs	calc obs	calc obs	calc obs.
Nq^3	2.7 (4)	13.5 (10)	4.0 (4)	23.8 (20)	6.5 (8)
$3Npq^4$	1.7 (3)	10.1 (15)	2.9 (6)	17.7 (22)	4.4 (9)
$6Np^2q^4$	2.4 (1)	9.8 (6)	3.4 (2)	19.2 (9)	5.8 (3)
$3Np^2q^2$	13.2 (8)	18.3 (23)	9.8 (5)	46.0 (59)	23.9 (13)
$6Np^3q^3$	5.5 (4)	9.2 (4)	4.7 (4)	22.8 (8)	10.8 (8)
$3Np^3q^2$	9.2 (3)	8.9 (4)	5.7 (4)	25.0 (7)	15.6 (7)
$3Np^3q$	30.9 (10)	17.1 (4)	13 6 (3)	54.6 (17)	44.9 (13)
Np^3	34.4 (67)	11.0 (32)	10.9 (27)	39.8 (107)	43.1 (94)

In reading this table one may remember that in the computed items the first two correspond to the first two frequencies, the next two are the two possibilities arising when there are two cases subsequent to the primary and that in particular the first of these two is the chain secondary-tertiary and the second is the direct double secondary, and finally the last four entries are the four possibilities when there are three subsequent cases listed in the order of diminishing chaining from the secondary-tertiary-quaternary through the secondary-double tertiary and the double secondary-tertiary to the final triple direct secondary.

We have one further case XIV for $m = 3$ for scarlet fever where we cannot identify the chains but may give the fit to the collected families. All children including primary cases are under 10 years, and all are supposedly susceptible, and are from families of any size provided there are no susceptibles 10 years of age or older. The frequencies for 0, 1, 2, 3 subsequent cases were 60, 21, 5, 1 for which series the fitted

values were 61.6, 17.9, 6.3, 1.2 with $q = 0.891$, $p = 0.109$, $r = 0.130$, $P = 0.65$.

It appears that our data for scarlet fever fit in the gross to the collected families about as well as our data for measles so that any inference which could be drawn from these fits for measles would be indicated also as sound for scarlet fever. In respect to the fits for measles one may remark that presumably the better fits should be expected in the cases which more rigorously exclude the immunes who could not be cases and appear to be ignored as agents of transmission in Greenwood's theory. Thus of the summation groups for $m = 3$ it seems as though XIII should be better than XII but the theory fits worse. Similarly, in the subgroups, IX and XI should be better than VIII and X but are not. Reverting to $m = 2$, VI should be better than V, but is not, and II and IV should be better than I and III respectively and the evidence is divided. The real difficulty, however, is that the theory however well it fits to the collected families seems not to fit at all when the families are subdivided along the lines demanded by the theory itself. It is not to be expected that two persons who should check over the Providence family records would come to the same enumeration of the number of instances of specified types of chaining within the families. Feeling, however, that the theory would give more chaining than was actual and wishing to give every benefit to the theory we deliberately classified the doubtful cases rather with the longer chains than with the shorter. We believe therefore that with the discrepancies so great as they are between them and prediction in respect to chaining we may definitely assert that the theory is non-applicable to Providence data for measles. We recognize that the epidemiology of measles in St. Pancras seems to be different from what it is in Providence, the secondary attack rates are not half so great and the values of p and q are also quite different; the real test of the theory by Greenwood would have consisted of determining whether the 57 two case families under A did divide satisfactorily into about 23 direct double secondaries

and about 35 secondary-tertiaries—and similarly for his case B and for the other cases listed in his paper.

50. *Types of Secondary Attack Rates.*—If some sort of chain theory is to be adopted the concept of secondary attack rate has to be subdivided and the fundamental assumption that there is a chance p of getting a case must be examined. Consider $m = 2$ and the summary cases V and VI. We may analyse V as follows: There were 79 families containing 158 susceptibles exposed to risk by the primary case none of whom developed a case. There were 113 families in which there were 226 susceptibles exposed to the primary case of whom 113 developed the disease and were thus direct secondaries. There were 459 families which developed two cases of which 396 had two direct secondaries and 63 had one direct secondary and a tertiary case. Now the total number of susceptibles exposed to the primary case was 1302 of whom $113 + 2 \times 396 + 63 = 968$ were direct secondaries leading to a *direct secondary attack rate* of $s = 0.743 \pm .012$ which is of course slightly less than the value $r = 0.792$ found when tertiary cases were included as secondaries. Now the number of susceptibles exposed to a secondary case were 113 in the families which had just one case secondary to the primary, plus 63 in the families which had a tertiary case or a total of 176 of whom 63 contracted the disease as tertiaries giving a tertiary attack rate of $t = 0.358 \pm 0.036$. On Greenwood's theory the direct secondary attack rates and the tertiary attack rates are the same and equal to p. The computed values of s and t differ by many times the standard error (of pure chance sampling) of that difference but depend to an unknown extent on the judgment with which the different families with two cases were assigned to the two possible types relative to chaining. For case VI we find $s = 0.807 \pm 0.012$ and $t = 0.569 \pm 0.049$. Both these rates are higher than for case V as might be expected owing to the more rigorous exclusion of immunes, and the rates are nearer together, differing by only some five times the chance error of the difference.

Turning next to $m = 3$ and cases XII and XIII, we may compute direct secondary attack rates of $s = 0.695 \pm 0.017$ and $s = 0.776 \pm 0.019$ respectively, the second being higher as might be expected because of the more rigorous exclusion of presumable immunes. The tertiary attack rates are respectively $t = 0.286 \pm 0.035$ and 0.475 ± 0.056 which are below the direct secondary attack rates by large amounts. Comparing the direct secondary attack rates for $m = 3$, with those for $m = 2$ we find here lower values but the individual differences are not significant. Comparing the tertiary attack rates for $m = 3$ with those for $m = 2$ we again find here lower values with the individual differences not significant. For $m = 3$ we may divide tertiary cases into two sorts, those arising from exposure to a single secondary and those arising from exposure to a double secondary. In case XII the two rates are respectively $t_1 = 0.337 \pm 0.049$ and $t_2 = 0.224 \pm 0.048$, the difference of 0.113 ± 0.069 is not significant (though it is interesting that the rate for exposure to a single secondary should be higher than for exposure to a double secondary); in case XIII the rates are $t_1 = 0.463$, $t_2 = 0.500$ and are statistically indistinguishable. Finally there is the quaternary attack rate for $m = 3$ which is 0.47 ± 0.12 for XII and 0.73 ± 0.13 for XIII. It is not surprising that the direct secondary, tertiary and quaternary rates for XIII should be higher than for XII because any immunes which may remain in the families entering into case XII must have the effect of reducing the tendency to chain. It is further interesting that although the tertiary rate is significantly less than the direct secondary rate, the quaternary rate is actually higher than the tertiary rate and for case XIII approaches the direct secondary rate. The quaternary rates are however very poorly determined statistically besides depending on interpretations that by nature have to be somewhat doubtful and, moreover, were actually interpreted to favor chaining.[25]

[25] If there were sufficient data one could discuss the effects of age on the direct secondary and the tertiary rates, etc., as we did for the overall secondary attack rate Such adjustments might somewhat change the new rates as they did the usual ones.

51. *The Number of Days Between Cases.*—How difficult it is to assign chains with any assurance may be apparent to anyone who has looked over a considerable number of family histories for any epidemic of measles. We may, however, give in Table 43 the distribution of 2nd and 3rd cases in families with 3 susceptibles and three cases, omitting all instances in which the first two cases occur on the same day, and tabulating in such a manner as to show the days to the cases of the younger and of the older of the two children. The numbers for the younger and older child at 9, 10, 11 days are practically identical, *i.e.*, the modal parts of the two distributions are the same, but there are more cases of the older child coming down less than 9 days after the primary than of the younger and more cases of the younger child coming down more than 11 days after the primary than of the older. The mean time is less for the older than for the younger. In case the middle child is the primary, the younger secondary is the youngest child and the older secondary is the oldest child, but if the youngest is the primary the younger secondary is the middle child and the older is the oldest, etc. We rearranged the data so as to ascertain among the secondaries the time in which the youngest, middle, and oldest child came down after the primary and the youngest took, on the average, a day more than either the middle or the oldest. (The secondary attack rates of the youngest and middle child were about the same and of the oldest considerably less—as might be expected because the oldest child should often be in those ages in which secondary attack rates are falling off.)

The fact that the younger child comes down more slowly than the older in this table led us to examine in a number of tables the correlation between the age of a child and the time in days he took to come down with measles after a primary case had entered the family. We examined children under 1 by age in months and by time to come down, also the younger child when a secondary in a two-susceptible family by age in years and by time to come down, and similarly the older child in the two-susceptible family; in no case did we

TABLE 43

Measles. Days Between Cases in Families With Three Susceptible Children. Three Children in the Family. Two Cases After the Case on the First Day

Younger	Older																														Total
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30+	
1	4			1	1		1		1	2		3							1	1	1										16
2	3	10	3	2				1		1					1			1		1											23
3	2	3	1	1	1	1		1			1																				11
4		2	2	5	1			1			1	1	1																	1	15
5	1	3	1	2	1	2			2	1																					13
6	2	1			3	6	2				1	1		1			1													1	19
7	1	3		1		1	17	1	1	1	2		1																		29
8	3			1	1	2	1	11	1					1																	21
9	3	3	3		1	1	5	8	36	3	2	2																			67
10	6	1	1	2			5	2	9	36	11	6						1			1										81
11	6	3	1	1	1			1	4	21	26	4	2	1	1				2												74
12	2		1	2	2	1	2		5	5	5	22	3	4	2							1									57
13	5		1		1	1	1	2	3	3	10	10	17			2											1				57
14	1	1			4	1	1	1	1	3	1	2	7	7	5	3	2			1	1										45
15	1							1		1	1	4	2	6	4	1										1					22
16			2	1								3	3	3	1	3	1														17
17	1									1	4		3	2										1			1				13
18								1		2		1			1																5
19			1				1												1											1	4
20									1	1					2																4
21												1		1																	2
22																				1											1
23									1				1									1									3
24								1			1	2	1																		5
25												1		2	1																4
26										1			1																		2
27										1			1	2																	1
28													1																		1
29											1		1			1															3
30+																					1	1								5	7
Total	41	30	17	19	17	16	36	32	65	83	70	63	45	30	18	10	4	2	4	4	4	3		1		1	2			8	625

find any perceptible correlation, the coefficients running between -0.06 and $+0.05$ and being in all cases less than their standard errors. Yet when this 3-susceptible family is divided into groups of youngest, middle and oldest child there is negative correlation between age of the child and time to come down with measles after the primary case as would be expected from the set up in Table 43. Thus we consider that there is no conclusion relative to correlation of age of child and time to come down with measles after exposure to be drawn from the conflicting evidence we have.

52. *Secondary Attack Rates in Various Family Types.*—We found above that the tertiary attack rate was greater when the exposure was to a single secondary than when it was to a double secondary. This would seem to be inherently improbable "other things being equal" but we have been unable to find what corrections or explanations should be made to resolve the paradox. It is a matter of interest to consider some multiple primary families and to compute the secondary attack rate (in the ordinary sense, not the direct secondary rate) for comparison with that of the single primary family. In the two-child family the secondary attack rate was $r = 0.769 \pm 0.015$ including all children under 1 year as susceptible; if those under 7 months who were not cases be excluded we found a rate of $r = 0.794 \pm 0.014$. If we take all families with more than 2 children but with only two susceptibles of whom one is the primary and including all children under 1 year as susceptible we have $r = 0.675 \pm 0.019$, a rate significantly lower than that found for the comparable group of 2-child families. The 3-child family with 3 susceptibles and with 2 primaries has a secondary attack rate of $r = 0.920 \pm 0.029$ (there were in this group no children under 1 year who escaped), which is a very high rate, significantly greater than any of the secondary rates just given. The 4-child family with two primaries and two other susceptibles (no escapes under 1 year of age) gives a rate of $r = 0.962 \pm 0.022$ which is even higher but not significantly so.

We may tabulate (Table 44) in more detail a variety of families for which we have computed secondary attack rates.

In all these cases children under 7 months of age were omitted unless they contracted measles. As the secondary attack rates vary with age the rates were adjusted and the adjusted figures are given in parenthesis. By and large it appears (1) that if we compare families with a given number of susceptibles which have no immunes with those of the same number of susceptibles which have some immunes, the secondary attack rates are higher in the former than in the latter for any given number of primaries (though, when the rates are age adjusted, the difference appears to vanish for the 3-susceptible families with 1 primary); and (2) that if we compare families with the same number of remaining susceptibles (*i.e.*, 3-susceptible families with 2 primaries and 2-susceptible families with 1 primary) the secondary attack rates are higher for the families which are larger (*i.e.*, have one more primary). It would not be difficult to think of possible a priori reasons why the presence of immunes in families of a given number of susceptibles might lower the secondary attack rate but only intensive and comparative study of actual families could give validity to such speculations.[29] So,

TABLE 44

MEASLES. UNADJUSTED AND ADJUSTED SECONDARY ATTACK RATES FOR VARIOUS TYPES OF FAMILIES

	Secondary Attack Rate
2-child, 2-susceptible family with 1 primary	80.6 ± 1.4 (77.6)
2-susceptible families of more than 2 children and 1 primary	73.7 ± 1.9 (70.0)
3-child, 3-susceptible families with 2 primaries	92.0 ± 2.9 (89.6)
3-susceptible families of more than 3 children and 2 primaries	77.4 ± 5.3 (76.4)
3-child, 3-susceptible families with 1 primary	84.0 ± 1.3 (79.6)
3-susceptible families with more than 3 children and 1 primary	81.9 ± 1.9 (79.9)
4-child, 4-susceptible families with 2 primaries	96.2 ± 2.2 (90.3)
4-susceptible families with more than 4 children with 2 primaries	82.7 ± 5.2 (83.4)
4-child, 4-susceptible families with 1 primary	87.4 ± 1.7 (83.6)
4-susceptible families of more than 4 children with 1 primary	73.7 ± 3.0 (73.1)
4-child or 4-susceptible families with 3 primaries, too few to be significant	

[29] Such an intensive study by houses was made by J. L. Halliday, "An Inquiry into the Relationship between Housing Conditions and the Incidence and Fatality of Measles," His Majesty's Stationary Office, Report Series Special No 120, 34 pp., 1928; but the total number of cases is very small and it is difficult to believe that much in the way of statistically sound conclusions could be found.

too, one might hazard a variety of guesses as to why the presence of additional primaries in families should make the secondary attack rate on a given number of susceptibles per family higher than for fewer primaries, but again substantiation would require detailed study of actual families, and this might reveal reasons totally different from any guessed in advance. We may recall that apparently the tertiary attack rate was diminished by the presence of double, as contrasted with single, secondaries; this would appear to be contrary to the finding that the presence of extra primaries increased the secondary attack rates. Such matters are of importance in the analysis of the mode of passage of measles within a family, but at present we cannot resolve the apparent clashes in the data, and, indeed, when we bear in mind that among real samples there are often found variations greater than those due to pure chance we cannot rely with assurance on the differences we find among the rates since we have only the pure chance standard deviations by which to judge differences.

53. *Further Note on Three-Child Family.*—There were 1581 two-child families of which 241 were double primary families and 1340 were single primary families. There were 1214 three-child families of which 70 had a triple primary, 154 a double primary and 990 a single primary. In the 990 single primary 3-child families the oldest was the case 292 times, the middle 273 times and the youngest 425 times. There were only 17 of the double primaries which involved the oldest and youngest, 74 which involved the oldest and the middle child, and 63 which involved the middle child and the youngest. Thus the oldest child brought the infection into the family 453 times, the middle one 480 times, and the youngest 575, numbers which are not equal, although the first two do not differ statistically. The mean ages and the standard deviations of the distributions for each child in the one primary families when a specified one was the primary case are given below:

Case	Mean Age of			Case	Standard Deviation for		
	Oldest	Middle	Youngest		Oldest	Middle	Youngest
Oldest	7.54	5.15	2.60	Oldest	2.16	1.94	1.94
Middle	11.90	6.49	3.20	Middle	4.70	2.09	2.05
Youngest	14.89	11.46	6.05	Youngest	4.28	4.11	2.65

Thus the mean age of the oldest child rises sharply when the primary runs through the series oldest, middle, youngest, and so do the mean ages of the middle and of the youngest child; the standard deviations also generally increase with the same sequence. A similar tabulation with much the same inferences may be given for scarlet fever:

Case	Mean Age of			Case	Standard Deviation for		
	Oldest	Middle	Youngest		Oldest	Middle	Youngest
Oldest	9.3	6.6	3.7	Oldest	3.9	3.5	3.1
Middle	11.5	7.7	4.6	Middle	4.5	3.2	3.0
Youngest	14.2	10.9	7.1	Youngest	4.7	4.3	3.4

The types of 3-child families which occur can be specified by sets of three of the four letters P for primary, S for secondary, E for susceptible escape, and I for immune, arranged in order of age, oldest, middle, youngest. The complete analysis of the 1214 three-child families in this manner is given in Table 45.

TABLE 45

MEASLES. FAMILY TYPES FOR THE 3-CHILD FAMILY

PPP	70										
PPS	52	PSP	14	SPP	13	PSS	207	PES	3	PIS	4
PPE	21	PEP	1	EPP	4	PSE	36	PEE	28	PIE	3
PPI	1	PIP	2	IPP	46	PSI	3	PEI	1	PII	7
SPS	71	SPE	5	SPI	3	SSP	18	SEP	1	SIP	0
EPS	18	EPE	16	EPI	1	ESP	2	EEP	8	EIP	2
IPS	111	IPE	42	IPI	6	ISP	26	IEP	25	IIP	343

It was interesting to note that: (1) If the oldest or youngest is a primary case and the other is a primary or secondary case, the chance of the middle child to escape is only 0.0207 ± 0.0090 which corresponds to a secondary attack rate of 97.93 ± 0.90 per cent. This rate is not well established because the numbers are so small but it is so very high that, despite its standard error, one is tempted to infer that even with all allowances the susceptible child between two cases is peculiarly liable to attack. (2) If the middle child escapes, the secondary attack rate is only 10 ± 4.7 per cent and would not be so large as 12 per cent even if all susceptible escapes under 1 year of age were rated as immunes. This is a very low rate; to some extent it may reflect the phenomenon of low secondary attack rates found for the 2-child family when the age difference between the children was great and thus be corroborative of that finding but chiefly it must be related to the fact that the middle child escapes. Indeed the anatomy of these families with respect to escape is interesting. If we take the three-child family we have a secondary attack rate of 76.6 for all susceptibles when all 1214 families are included. There is no secondary attack rate for the 70 triple primary families; for the 154 double primary families the secondary attack rate is 75.2 ± 4.2 while for the single primary families it is 76.8 ± 1.3—and if these single primary families are divided into those which contain no immune and those which contain one, the secondary attack rates among susceptibles are 79.5 ± 1.4 and 66.5 ± 3.2 respectively (none of these rates are adjusted for the presumable immunes under 1 year of age, viz., those under 7 months who do not contract the disease). If next we look to the single primary families which have one specified susceptible escape, we find the secondary attack rate on the other susceptible to be 55.6 ±.4.6, and thus the presence of an escape lowers the secondary attack rate more than the presence of an immune. The details are given below:

SECONDARY ATTACK RATES IN ONE PRIMARY FAMILIES

	Oldest	Middle	Youngest	Total
One Immune	67±3.3	44±17	75±15	66.5±3.2
One Escape	45±7.4	10±5	48±5.4	55.6±4.6

APPENDIX I. THE TWO PARTS OF THE 1931–2 MEASLES EPIDEMIC

54. *Distribution of Families.*—The course of measles in Providence 1929–1934 was characterized by a low incidence in all years but 1931 and 1932 and a major epidemic in those years. The distribution by years is in Table 46.

TABLE 46

Year	1929	1930	1931	1932	1933	1934	Total
Families	663	50	2,020	2,781	14	137	5,665
Primaries	775	59	2,396	3,342	16	153	6,741
Secondaries	360	43	1,471	1,488	10	89	3,461
Cases	1,135	102	3,867	4,830	26	242	10,202
Secondaries/Primaries	0.465	0.729	0.614	0.445	0.625	0.582	0.513
Primaries/Families. .	1.169	1.180	1.186	1.202	1.143	1.117	1 190

In making the table the cases secondary to a primary have been classified as of the year in which the (first) primary occurred. The course in 1931 consisted of a moderate epidemic in the late Spring falling to a score of cases in September and a sharp rise to epidemic proportions. For this reason we consider that the behavior of measles in 1931–1932 may properly be characterized as a major epidemic seasonally interrupted rather than as two epidemics, one in the late Spring of 1931 followed by another in the Fall of 1931 and Winter of 1932. Hence we shall speak of 1931 as the first and 1932 as the second part of the great 1931–1932 epidemic which contained about six sevenths of all the measles in the six year period 1929–1934. The ratio of secondary to primary cases changes from year to year by more than can be attributed to chance; it is quite high in 1931 and decidedly

low in 1932. The ratio of primaries to families also changes being apparently low in the years in which there is little measles—the difference between the figures for 1931 and for 1930 being not significant.

In a year in which measles is very rare as in 1933 or even in 1930 it would clearly be unlikely that there should be a fair sample of different sizes of family. Furthermore the distribution of families by size may really change from year to year; we give the distribution in Table 47. That there is a notable difference in the distribution in the two big years stands out at once; in 1931 there were relatively few 1- and 2-child families attacked as compared with 1932 and relatively more large families. On the other hand there appears to be no significant difference between the distributions for the whole 1931–32 epidemic and that for the other four years.

TABLE 47

MEASLES. DISTRIBUTION OF FAMILIES BY SIZE IN EACH YEAR

Size	1929	1930	1931	1932	1933	1934
1	135	9	306	612	4	20
2	184	6	525	821	3	42
3	149	8	453	576	1	27
4+5	134	12	497	518	5	29
6+7+8	56	12	211	212	0	14
9+	5	3	28	42	1	5
Total	663	50	2,020	2,781	14	137

For 1931 and 1932 we may give a further breakdown by size cf family. It is to be observed that in all cases the ratio

	1931					1932				
	2	3	4–5	6–8	9+	2	3	4–5	6–8	9+
Families	525	453	497	211	28	821	576	518	212	42
Primaries	590	559	643	265	33	959	730	672	302	67
Secondaries	231	374	523	290	53	328	374	462	245	79
Cases..	821	933	1,166	555	86	1,287	1,104	1,134	547	146
Sec./Pri	0.392	0.669	0.813	1.09	1.61	0.342	0.512	0.688	0.811	1.18
Pri./Fam.	1.124	1.234	1.294	1.26	1.18	1.168	1.267	1.297	1.42	1.60

of secondaries to primaries is less in 1932 than in 1931 as it was in totals and further that the ratio of primaries to families is greater in 1932 than in 1931.

55. *Mean Ages and Standard Deviations.*—Tables 48*a*, *b*, *c*, 49*a*, *b*, *c*, 50*a*, *b*, *c* give the age distribution by size of family for primary cases, for secondary cases, and for susceptibles, excluding primary cases, in 1931, 1932, and in the other 4 years taken together.

The mean age of all primaries is the same in 1931 and 1932, moreover the mean ages in the different sizes of family are the same within minor statistical fluctuations. The mean age of primary case in the other four years is, however, more than half a year less—a statistically significant difference. Moreover although the mean ages in the columns of totals are so nearly alike in 1931 and 1932 the age distributions in these columns are by no means the same, that for 1932 having a decidedly larger standard deviation than that for 1931. (A Chi-square test shows that the distributions are very different.) The center of the distribution is higher and the ends

TABLE 48*a*

MEASLES. AGE DISTRIBUTION BY SIZE OF FAMILY FOR PRIMARY CASES 1931

Age	1	2	3	4–5	6+	Total
<1	7	8	8	13	6	42
1	10	29	14	22	7	82
2	19	29	28	37	15	128
3	13	40	34	38	13	138
4	14	35	49	45	28	171
5	35	64	67	84	40	290
6	50	117	93	124	62	446
7	69	108	125	134	57	493
8	39	83	93	89	37	341
9	27	43	25	29	24	148
10	10	23	17	15	6	71
11	10	8	3	8	2	31
12–14	2	3	1	4	1	11
15–21	1		2	1		4
Total	306	590	559	643	298	2,396
Mean Age	6.74	6.49	6.51	6.37	6.43	6.48
St. Dev.	2.63	2.46	2.34	2.41	2.25	2.42

TABLE 48*b*

MEASLES. AGE DISTRIBUTION BY SIZE OF FAMILY FOR SECONDARY CASES 1931

Age	2	3	4+5	6+	Total
<1	4	20	24	23	71
1	25	41	63	40	169
2	27	37	67	50	181
3	27	56	83	44	210
4	44	64	76	42	226
5	40	49	63	46	198
6	23	33	46	23	125
7	16	22	29	15	82
8	6	16	29	25	76
9	5	16	24	12	57
10	5	12	9	10	36
11	7	5	7	7	26
12–14	2	2	2	5	11
15–21		1	1	1	3
Total	231	374	523	343	1,471
Mean Age	4.97	4.85	4.71	4.82	4.81
St. Dev.	2.59	2.79	2.72	3.06	2.80

TABLE 48*c*

MEASLES AGE DISTRIBUTION BY SIZE OF FAMILY FOR SUSCEPTIBLES EXCLUDING PRIMARY CASES IN 1931

Age	2	3	4+5	6+	Total
<1	16	49	79	55	199
1	30	54	74	49	207
2	31	46	76	57	210
3	34	63	91	55	243
4	53	74	81	49	257
5	46	56	69	49	220
6	27	40	54	26	147
7	17	28	33	21	99
8	12	20	35	27	94
9	6	19	32	21	78
10	6	16	12	13	47
11	12	7	14	14	47
12–14	8	4	11	22	45
15–21	2	5	11	12	30
Total	300	481	672	470	1,923

TABLE 49*a*

MEASLES. AGE DISTRIBUTION BY SIZE OF FAMILY FOR PRIMARY CASES 1932

Age	1	2	3	4–5	6+	Total
<1	7	19	13	20	8	67
1	25	28	22	20	20	115
2	30	50	41	36	18	175
3	37	71	57	41	34	240
4	42	87	68	49	28	274
5	89	147	107	87	37	467
6	119	177	122	129	73	620
7	91	141	114	105	59	510
8	70	101	92	87	52	402
9	46	59	45	61	24	235
10	17	32	21	19	8	97
11	12	23	11	8	2	56
12–14	19	12	15	8	4	58
15–21	8	12	2	2	2	26
Total	612	959	730	672	369	3,342
Mean Age	6.75	6.50	6.41	6.48	6.27	6.50
St. Dev.	2.96	2.85	2.64	2.63	2.68	2.77

TABLE 49*b*

MEASLES. AGE DISTRIBUTION BY SIZE OF FAMILY FOR SECONDARY CASES 1932

Age	2	3	4–5	6+	Total
<1	11	21	26	25	83
1	37	47	58	38	180
2	52	53	60	47	212
3	51	49	70	48	218
4	50	68	72	55	245
5	36	54	68	37	195
6	19	30	25	27	101
7	20	11	28	13	72
8	14	11	26	12	63
9	6	11	11	9	37
10	12	7	10	5	34
11	7	7	3	5	22
12–14	9	5	4	2	20
15–21	4		1	1	6
Total	328	374	462	324	1,488
Mean Age	5.01	4.51	4.55	4.39	4.61
St. Dev.	3.35	2.71	2.71	2.74	2.87

TABLE 49*c*

MEASLES. AGE DISTRIBUTION BY SIZE OF FAMILY FOR SUSCEPTIBLES EXCLUDING PRIMARY CASES IN 1932

Age	2	3	4+5	6+	Total
<1	35	64	65	65	229
1	48	54	62	45	209
2	63	63	65	53	244
3	59	53	79	52	243
4	62	73	83	64	282
5	39	62	77	42	220
6	21	34	29	34	118
7	22	20	33	17	92
8	15	15	33	19	82
9	9	17	20	16	62
10	16	11	15	6	48
11	8	9	9	10	36
12–14	14	10	15	9	48
15–21	11	4	6	9	30
Total	422	489	591	441	1,943

TABLE 50*a*

MEASLES. AGE DISTRIBUTION BY SIZE OF FAMILY FOR PRIMARY CASES—OTHER THAN 1931 AND 1932

Age	1	2	3	4+5	6+	Total
<1	6	5	9	3	6	29
1	12	18	19	10	6	65
2	16	24	11	16	7	74
3	11	26	22	18	10	87
4	12	35	24	25	6	102
5	23	28	25	28	15	119
6	29	43	40	51	31	194
7	23	49	35	37	16	160
8	9	17	23	22	8	79
9	4	12	4	5	4	29
10	8	4	1	8		21
11	7	7	2	2		18
12–14	4	1	2	6	2	15
15–21	4	4	2	1		11
Total	168	273	219	232	111	1,003
Mean Age	6.24	5.89	5.65	6.19	5.66	5.94
St. Dev.	3.54	2.97	2.83	2.68	2.53	2.94

TABLE 50*b*

MEASLES. AGE DISTRIBUTION BY SIZE OF FAMILY FOR SECONDARY CASES OTHER THAN 1931 AND 1932

Age	2	3	4+5	6+	Total
<1	10	7	8	7	32
1	4	21	20	15	60
2	12	23	24	20	79
3	10	18	30	12	70
4	16	20	30	18	84
5	15	18	23	15	71
6	5	12	12	4	33
7	7	6	6	3	22
8	5	7	8	7	27
9	2		5	4	11
10	1	2	1	2	6
11			2	1	3
12–14			2		2
15–21		1		1	2
Total	87	135	171	109	502
Mean Age	4.45	4.20	4.44	4.39	4.37
St. Dev.	2.44	2.58	2.54	2.91	2.62

TABLE 50*c*

MEASLES. AGE DISTRIBUTION BY SIZE OF FAMILY FOR SUSCEPTIBLES EXCLUDING PRIMARY CASES OTHER THAN 1931 AND 1932

Age	2	3	4+5	6+	Total
<1	19	19	34	20	92
1	5	26	25	18	74
2	14	28	26	23	91
3	15	23	35	18	91
4	19	24	31	22	96
5	18	21	30	20	89
6	7	15	17	5	44
7	7	10	9	3	29
8	7	7	11	10	35
9	4	3	7	7	21
10	2	2	5	5	14
11			5	2	7
12–14			7	4	11
15–21	1	4	2	4	11
Total	118	182	244	161	705

lower in the first part of the epidemic as compared with the second part. Turning to the secondary cases we find the mean age in 1932 slightly less than in 1931, but in both years the mean age is greater than in the other years (as with the primaries).

56. *Secondary Attack Rates.*—These are given in Tables 51, 52, 53, for 1931, 1932, and the other 4 years respectively. The secondary attack rates seem to be a trifle less in the earlier part than in the later part of the epidemic whether on the totals or by size of family and this difference seems not altered by age adjustment.[30] The secondary attack rates are essentially the same for families of different sizes, except for those of six or more children where they are less, in both parts of the epidemic. There is a distinct indication on the totals that for susceptibles under 4 years and above 10 years of age the secondary attack rates are lower, for those from 4 to 10 years inclusive are higher in the first part than in the second part of the epidemic; by size of family the rates because of small numbers are decidedly erratic.

TABLE 51

MEASLES. SECONDARY ATTACK RATE BY AGE AND SIZE OF FAMILY 1931

Age	2	3	4+5	6+	Total
<1	25.0	40.8	30.4	41.8	35.7
1	83.3	75.9	85.1	81.6	81.6
2	87.1	80.4	88.2	87.7	86 2
3	79.4	88.0	91.2	80.0	86.4
4	83.0	86.5	93.8	85.7	87.9
5	87.0	87.5	91.3	93 9	90.0
6	85.2	82.5	85.2	88.5	85.0
7	94.1	78.6	87.9	71.4	82.8
8	50 0	80 0	82.9	92.6	80.9
9	83.3	84 2	75.0	57.1	73.1
10	83.3	75.0	75.0	76.9	76.6
11	58.3	71.4	50.0	50.0	55.3
12–14	25.0	50 0	18.2	22.7	24.4
15–21	0 0	20.0	9.1	8.3	10.0
Total	77.0	77 8	77.8	73.0	76.5

[30] This conclusion seems to be opposite to that of Halliday.[28]

TABLE 52

MEASLES. SECONDARY ATTACK RATE BY AGE AND SIZE OF FAMILY 1932

Age	2	3	4+5	6+	Total
<1	31.4	32.8	40.0	38.5	36.2
1	77.1	87.0	93.5	84.4	86.1
2	82.5	84.1	92.3	88.7	86.9
3	86.4	92.5	88.6	92.3	89.7
4	81.0	93.2	86.7	85.9	86.9
5	92.3	87.1	88.3	88.1	88.6
6	90.5	88.2	86.2	79.4	85.6
7	90.9	55.0	84.8	76.5	78.3
8	93.3	73.3	78.8	63.2	76.8
9	66.7	64.7	55.0	56.2	59.7
10	75.0	63.6	66.7	83.3	70.8
11	87.5	77.8	33.3	50 0	61.1
12–14	64.3	50.0	26.7	22.2	41.7
15–21	36.4	0.0	16.7	11.1	20.0
Total	77.7	76.5	78.2	73.5	76.6

TABLE 53

MEASLES. SECONDARY ATTACK RATES BY AGE AND SIZE OF FAMILY OTHER THAN 1931 AND 1932

Age	2	3	4+5	6+	Total
<1	52.6	36.8	23.5	35.0	34 8
1	80.0	80.8	80.0	83.3	81.1
2	85.7	82.1	92.3	87.0	86.8
3	66.7	78.3	85.7	66.7	76.9
4	84.2	83.3	96 8	81.8	87.5
5	83.3	85.7	76.7	75.0	79.8
6	71.4	80.0	70.6	80.0	75.0
7	100.0	60.0	66.7	100.0	75.9
8	71.4	100.0	72.7	70.0	77.1
9	50.0	0.0	71.4	57.1	52.4
10	50.0	100.0	20.0	40.0	42.9
11	0.0	0.0	40.0	50.0	42.9
12–14	0.0	0.0	28.6	0.0	18.2
15–21	0.0	25.0	0.0	25.0	18.2
Total	73.7	74.2	70.1	67.7	71.2

APPENDIX II. LOCALNESS OF MEASLES

57. *Measles in the Wards of Boston and in Cities Nearby.*—We have treated Providence as a whole without subdivision into wards or other regions. Such a method is natural to a health administrator; he has a certain number of cases with

which to deal. We are not able to subdivide Providence but we can get reported cases of measles in Boston by wards, although we cannot get the figures by months and by wards.[31] Tables 54, 55, 56 give respectively the case rates per thousand population age 15 and under for the 26 wards of Boston 1916–1925 (using 1920 populations), for the 22 wards of Boston 1926–1936 (using 1930 populations) and for Boston and some of its suburbs for 1926–1935 (using also 1930 populations).

TABLE 54

MEASLES CASE RATES BY CALENDAR YEAR 1916 THROUGH 1925 IN THE 25 WARDS OF THE CITY OF BOSTON *

	1925	1924	1923	1922	1921	1920	1919	1918	1917	1916	Mean	σ	C.V.	% Pop
1	34.1	7.2	18.1	8.3	24.8	17.6	1.3	19.9	15.7	48.6	19.6	13.1	67	31.7
2	25.9	4.5	19.3	13.5	15.9	25.2	2.7	13.6	19.3	44.2	18.4	11.2	61	39.3
3	19.0	55.4	7.2	23.8	2.4	17.0	1.2	19.7	12.0	24.5	18.2	14.7	81	31.4
4	24.7	47.9	3.4	51.5	4.9	38.7	2.7	69.7	17.3	28.3	28.9	21.7	75	28.3
5	24.3	17.4	9.7	34.2	11.8	22.2	15.6	16.0	35.7	20.5	20.7	8.3	40	36.0
6	42.2	34.1	69.3	13.7	22.8	33.3	15.5	43.7	72.9	26.7	37.4	19.3	52	21.2
7	36.2	26.1	52.1	35.6	25.2	63.1	7.8	55.8	33.6	25.5	36.1	15.8	44	9.4
8	21.4	33.6	18.4	23.3	12.9	52.9	6.0	44.6	40.5	24.6	27.8	14.0	50	14.5
9	21.2	23.6	6.9	18.6	11.7	19.0	1.6	22.1	20.0	16.9	16.2	6.8	42	35.3
10	46.3	31.8	6.9	26.8	12.1	33.7	2.1	37.0	16.3	26.5	24.0	13.4	56	30.0
11	19.8	28.6	10.2	20.2	17.2	22.0	1.7	21.5	14.5	30.7	18.6	8.0	43	31.0
12	31.6	25.3	38.6	15.3	26.9	29.4	9.1	46.0	23.3	20.1	26.6	10.2	38	30.7
13	36.0	18.6	37.8	23.9	22.7	28.1	4.0	39.6	23.6	42.2	27.6	11.1	40	25.9
14	31.5	11.0	28.6	16.3	18.3	23.1	4.1	34.4	19.8	19.2	20.6	8.8	43	29.2
15	44.3	16.0	26.3	19.5	35.9	30.7	6.4	30.4	13.6	14.9	23.8	11.1	47	28.9
16	57.5	12.3	57.2	17.2	24.4	31.7	39.8	23.3	29.4	30.4	32.3	14.5	45	25.2
17	34.3	35.1	27.6	33.0	13.0	42.6	12.2	41.8	32.6	37.3	31.0	10.1	33	27.9
18	57.4	12.5	29.3	30.1	13.2	38.6	12.7	34.5	26.1	29.1	28.4	13.1	46	29.7
19	42.3	54.7	34.0	26.4	20.0	59.8	12.2	41.0	36.1	27.5	35.4	14.0	40	25.3
20	27.9	26.4	26.6	28.3	19.5	59.9	3.4	23.3	40.1	8.1	26.4	15.0	57	28.8
21	20.1	24.3	31.6	25.1	14.5	55.6	4.1	31.0	40.9	18.1	26.5	13.7	51	29.1
22	42.3	22.9	13.7	46.6	9.0	23.4	2.5	53.9	15.9	7.8	23.8	16.9	71	27.3
23	25.2	7.8	50.7	20.4	7.8	37.3	8.8	38.3	32.1	22.5	25.1	13.9	55	26.9
24	20.2	5.9	10.1	28.0	11.4	10.7	33.0	9.0	8.4	35.5	17.2	10.5	61	33.6
25	36.0	54.9	18.9	66.2	8.6	21.8	59.8	24.5	41.2	6.1	33.8	20.2	60	18.5
26	24.5	20.9	8.7	30.7	10.0	7.3	20.7	20.7	18.2	6.9	16.9	7.8	46	32.0
Mean	32.5	25.3	25.4	26.8	16.0	32.5	11.2	32.9	26.9	24.7				
σ	10.9	14.8	17.1	12.4	7.6	15.1	13.5	14.2	13.5	11.0				
C.V.	34	58	67	46	47	46	121	43	50	44				

* σ indicates the standard deviation, C.V. the coefficient of variation in percentage and % Pop. the percentage of population 15 years and under in 1920.

[31] It would have been more satisfactory to have the figures by epidemic years, say from August 1 to July 31, if we could have obtained them.

TABLE 55

MEASLES CASE RATES BY CALENDAR YEAR 1926 THROUGH 1936 IN THE 22 WARDS OF THE CITY OF BOSTON *

	1936	1935	1934	1933	1932	1931	1930	1929	1928	1927	1926	Mean	σ	C.V.	% Pop
1	18.4	2.7	20.3	38.6	0.8	13.5	22.5	4.3	23.5	24.8	8.8	16.2	10.9	67	35.7
2	47.6	1.9	22.1	32.3	19.8	7.8	23.4	23.5	39.8	10.8	26.6	23.2	12.9	56	28.4
3	28.3	8.8	23.8	18.7	16.8	6.2	28.4	5.1	25.2	28.0	31.8	20.1	9.2	46	27.6
4	13.1	25.5	26.7	12.7	43.1	20.1	108.4	11.9	118.3	27.9	84.2	44.7	37.8	85	7.9
5	28.8	8.1	53.0	14.9	31.9	24.8	50.2	10.5	74.4	36.3	40.0	33.9	19.1	56	10.5
6	43.7	8.2	31.2	61.9	3.3	6.9	54.6	1.5	14.4	24.6	8.9	23.6	20.5	87	31.9
7	13.3	4.1	7.4	13.9	4.0	4.6	11.0	0.4	9.9	27.8	14.4	10.1	7.1	71	32.8
8	59.7	11.3	26.5	23.9	37.0	7.5	58.2	3.5	39.0	21.4	32.4	29.1	17.8	61	28.9
9	17.0	5.7	31.6	10.3	33.1	7.3	72.5	2.9	45.4	15.2	29.5	24.6	19.9	81	23.0
10	36.4	5.0	27.9	14.7	22.2	7.9	59.4	5.1	34.9	20.7	23.7	23.4	15.4	66	29.1
11	39.4	3.9	39.4	11.1	10.6	6.9	42.4	1.8	31.5	29.2	14.8	21.0	14.8	70	29.7
12	89.1	6.2	61.9	30.3	17.0	10.7	65.7	2.8	35.0	40.3	16.0	34.1	26.6	78	24.8
13	38.4	7.7	35.0	32.0	10.2	4.5	51.3	1.8	34.3	20.9	22.7	23.5	15.3	65	27.0
14	40.8	6.7	58.2	20.6	13.7	7.3	34.7	3.4	46.4	29.3	21.4	25.7	17.0	66	26.1
15	46.2	3.8	11.9	36.1	7.1	1.1	42.6	3.3	52.1	11.8	14.4	20.9	18.4	88	28.4
16	55.5	6.6	59.5	22.0	13.9	12.2	50.6	1.7	70.1	13.3	8.9	28.6	23.8	83	26.4
17	58.7	6.7	31.2	40.6	20.6	10.4	44.7	1.7	61.6	17.5	28.1	29.3	19.4	66	25.9
18	16.4	4.2	39.9	9.0	5.9	8.9	14.1	1.6	50.5	6.3	9.3	15.1	14.9	99	31.5
19	27.0	5.7	92.7	17.6	7.9	5.7	85.5	3.0	68.1	10.3	19.2	31.2	32.4	104	24.4
20	11.5	6.1	41.9	47.9	8.1	4.4	55.5	4.1	37.2	8.4	22.8	22.5	18.6	83	25.3
21	46.6	5.5	54.6	19.0	9.2	39.4	41.9	4.7	61.9	14.7	62.6	32.7	21.6	66	13.5
22	39.1	6.3	60.6	10.5	11.6	65.0	20.2	4.0	38.9	20.0	11.7	26.2	20.6	79	29.4
Mean	37.0	6.8	39.0	24.5	15.8	12.9	47.2	4.7	46.0	20.9	25.1				
σ	18.6	4.6	19.5	13.6	11.2	14.0	22.9	4.9	23.1	8.9	17.8				
C.V.	50	67	50	56	71	109	49	105	50	43	71				

* σ indicates the standard deviation, C.V. the coefficient of variation in percentage and % Pop the percentage of population 15 years and under in 1930.

TABLE 56

MEASLES CASE RATES BY CALENDAR YEARS 1926 THROUGH 1935 IN BOSTON AND NEARBY CITIES *

	1935	1934	1933	1932	1931	1930	1929	1928	1927	1926	Mean	σ	C.V.	% Pop.
Boston	6.1	37.3	25.5	13.3	11.7	41.8	4.3	39.9	21.4	20.9	22.2	13.1	59	26.4
Cambridge	4.5	68.1	20.1	13.1	13.6	46.3	16.1	68.4	16.2	24.1	29.0	22.1	76	27.1
Somerville	3.2	28.7	3.4	8.9	7.6	28.2	5.1	29.5	6.3	19.4	14.0	10.6	75	27.8
Newton	9.8	49.0	14.2	74.4	9.2	41.9	5.0	97.4	5.8	95.1	40.2	35.4	88	26.3
Malden	3.6	35.6	7.2	13.3	31.1	16.8	3.3	54.0	1.4	21.7	18.8	16.2	86	27.9
Medford	4.0	40.5	10.7	28.9	14.2	35.1	6.3	58.4	15.3	21.1	23.4	16.3	70	29.8
Brookline	5.7	71.4	5.3	15.1	20.0	61.3	8.3	58.4	22.0	35.6	30.3	23.6	78	19.2
Everett	3.6	19.0	0.9	1.5	18.7	19.3	1.8	27.0	5.4	12.8	11.0	9.0	82	30.4
Chelsea	3.6	19.1	0.8	1.9	31.3	8.9	2.9	21.6	10.1	14.1	11.4	9.5	83	30.8
Revere	0.8	25.7	0.8	1.1	37.4	12.6	2.0	19.5	22.3	8.1	13.0	12.1	93	33.1
Waltham	1.9	26.7	20.6	5.3	3.4	60.9	7.0	27.4	6.1	24.0	18.3	17.2	94	29.1
Mean	4.3	38.3	10.0	16.1	18.0	33.9	5.6	45.6	12.0	27.0				
σ	2.3	17.2	8.5	20.0	10.4	17.5	3.8	23.0	7.3	22.6				
C.V.	53	45	85	124	58	52	68	50	61	84				

* σ indicates the standard deviation, C.V. the coefficient of variation of percentage and % Pop the percentage of population 15 years and under in 1930.

In these tables it is immediately noticeable that in respect to epidemics the different wards of Boston are not acting in

unison.[32] For example, judging by the mean of the rates, the incidence for measles in the first period was greatest in 1918, yet for Ward 24 the rate in that year was only about half the average of the rates for that ward for the 10 years and only 5 of the 26 wards had their highest rates for the 10 years in 1918. The means of the rates (for all wards) vary from 11.2 to 32.9 as may well be expected for a disease so variable as measles from year to year, but the standard deviations vary from 10.9 to 17.1 and the coefficients of variation from 34 to 121. Thus, speaking descriptively and without reference to sampling errors, we see that there is a high degree not only of variability in the mean from year to year but of variability both of the standard deviation and of the coefficient of variation. In the low year 1919, most wards were low but Ward 25 was very high. If the later series for Boston be examined much the same situation is discovered; not only is the mean rate variable but the standard deviations and coefficients of variation vary. In some years the incidence is spread much more uniformly over the city than in others.

If the table be examined with respect to the variability of the incidence of (reported) measles for the decade 1916–1925 for the 26 wards it is seen that there are high wards and low wards but with great annual variability from their mean. The ward means for the decade vary from 16.2 to 37.4 and the standard deviation of these means about the overall mean 25.4 is 6.2 giving a coefficient of variation of 24 per cent whereas the standard deviation of the annual means about 25.4 is 6.7 giving a coefficient of variation of 26 per cent. Thus, as reported, measles was approximately as variable over the wards as over the years.

It is desirable to have some estimate of the variability when allowance is made both for the prevalence of measles by years and for the prevalence of measles by wards. A rough estimate will be satisfactory because for a disease so

[32] Stocks and Karn have divided St. Pancras into half mile squares in which they know the number of cases of measles by 10 day periods from March 23, 1924, through March 28, 1927. Among other things they comment on the localness of the epidemic

variable as measles a ten year period cannot be expected to establish a good mean either for the individual wards or for the city as a whole. One may thus take the mean rate for each year as the standard for that year and the mean rate for each ward as a standard for that ward and compute the standard or normal for each ward each year by multiplying these (marginal) means together and dividing by the overall mean (25.43). In this way one may tell how much the actual rate for any ward in any year exceeds or falls below the standard or normal or, as it is usually called, the expected value. The mean of the 260 values for the excess above expected (or deficiency below it) will of course be zero: the standard deviation figures out to be 11.6 giving a coefficient of variation when referred to the original mean 25.43 of 46 per cent which is nearly twice the value for the years or for the wards.[33] The standard deviation of the 260 case rates was 14.9 giving a coefficient of variation of 57 per cent. If the numbers of cases involved were small the coefficient of variation of 46 per cent might be due largely to pure chance fluctuation in the numbers of cases per ward per year and any comparison with the values for the marginal means (which would naturally be statistically more stable because of involving much larger numbers of cases) might be illusory; but actually the numbers of cases are sufficiently large so that the contribution of the pure chance fluctuations to the value 46 per cent is practically negligible.[34] In this manner we get

[33] A more rigorous treatment than the above could be made by resort to the method of variance analysis developed by R. A. Fisher. The calculations have been carried through on that basis with results not notably different from those obtained by the "rough" method. More important for our purposes than a rigorous treatment of the instance is the rough treatment of several instances. The results for the 11-year period 1926–1936 (Table 55) are: standard deviation of the 242 rates 21.3, C.V. = 84 per cent; standard deviation by years 14.3, C.V. = 56 per cent; standard deviation by wards 7.3, C.V. = 29 per cent; standard deviation of excess or deficiency by ward by year 13.2, C.V. = 52 per cent. The results for the decade 1926–1935 for Boston and suburbs (Table 56) are standard deviation of the 110 rates 20.4, C.V. = 97 per cent; standard deviation by years 13.6, C.V. = 65 per cent; standard deviation by city 8.8, C.V. = 42 per cent; standard deviation of excess or deficiency by city by year 10.8, C.V. = 51 per cent.

[34] We estimate, again roughly, the standard deviation of pure chance fluctuations as 1.8 yielding a coefficient of variation, if it alone were present, of 7 per

a rough estimate of the "localness" of the incidence of measles; if the distribution over the wards each year did not vary from that "expected" there would be no standard deviation of excess or deficiency and the coefficient of variation would be 0 instead of 46 per cent.

The remarkably regular biennial periodicity upon which many people have commented is not especially in evidence in Boston; 1936 and 1934 were high but 1932 was not, 1930 and 1928 were high but 1926, 1924, 1922 were about average. When one looks at the individual wards one finds similar departures from periodicity. Thus in Ward 4 there was a large incidence of measles in 1926, 1928, 1930 but the incidence remained low from 1931 to 1936. As the number of wards and hence necessarily the ward-lines were changed between 1925 and 1926 it is impossible to compare incidences on the same areas over the whole 21-year period, and in the absence of monthly records a detailed analysis is perhaps not worth while anyhow. Yet the way in which measles moves around the city with sometimes quite local outbreaks may seem to throw some doubt on the applicability of Stocks and Karn's [3] theory explaining the periodicity by a temporary immunity acquired by children exposed in the family to measles but not contracting it. Of course, their secondary attack rates are so much lower than ours that such a temporary immunity might manifest itself more clearly on their statistics than on ours.

The percentage of the population 15 years and under relative to the total population in the different wards varies from under 10 to nearly 40; it also varies as between Boston and its suburbs. A plot of the rate of incidence of measles as reported against the percentage of the population 15 or younger shows a marked negative correlation. The calculation of the correlation coefficient for the 10-year (or 11-year) mean rates in Tables 54, 55, 56 with the percentage 15 or younger gives respectively $r = -0.76$, -0.82, -0.65.

cent: if we deduct the square of this from the square of 46 per cent and extract the square root we have a coefficient of variation of the fluctuations other than those due to chance of 45.5 per cent.

These correlations are high enough and consistent enough to indicate a very significant relationship. Where the children are a relatively large fraction of the population the reported incidence is relatively lower. This is presumably a finding in opposition to Farr's Law by virtue of which the rates should be higher where the density of the child population is greater —as we may presumably assume that the density of children is greater when the percentage of children is higher. Hence the reality of the phenomenon should be accentuated rather than diminished. We suppose that the meaning of these high negative coefficients should be sought in the completeness or incompleteness of reporting rather than in the true incidence of measles in the population. [If the correlation of the measles rates in the wards (1916–1925) with the persons per family (as reported by the U. S. Census) be calculated the result is -0.16 which is not significant but still has the sign opposite to the one that might be expected if reporting were equally good in all wards.]

For each of the ten years 1916–1925 the correlation was computed between the measles rates in the wards and the percentage of population 15 and under (as given for 1920, no satisfactory values for each of the years individually being obtainable). The results were $+0.16$, -0.52, -0.56, -0.24, -0.56, -0.19, -0.31, -0.50, -0.37, -0.26 with a mean of -0.34. These values are all numerically smaller than that obtained when the 10-year mean was used ($r = -0.76$) as should be expected because of the inherently greater variability of the yearly values and might indicate that if one had a longer series the correlations with the means should be even higher than were found for the 10-year period. Such correlations upon a variable (percentage 15 years and under) will necessitate a resort to partial correlations if we attempt to intercorrelate the rates in the different wards for different pairs of years. It is but another indication of the difficulties and unsatisfactoriness of making epidemiological inferences from reported cases in the absence of ability to correct for the incompleteness of reporting and indicates that

one of the most important next steps in developing the epidemiology of the infectious diseases of childhood on the basis of health department records is to obtain a better estimate of the inadequacy of reporting and of the incidence of that inadequacy relative to a variety of variables such as age, size of family, density of population, etc., which we may wish to treat in our epidemiological discussion.[35]

[35] After this manuscript was completed an article by Franklin H. Top, "Measles in Detroit, 1935, I, Factors Influencing the Secondary Attack Rate Among Susceptibles at Risk," appeared in the *Am. Jour. Pub. Health*, **28**, 935–943, 1938.

AGRICULTURE AND CURRENT POPULATION TRENDS

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THE major facts concerning the contributions of the farm population to the Nation's population growth are easily summarized. Reproduction rates in the farm population indicate an excess of approximately two-thirds above replacement needs per generation and the farm population, which includes about one-fourth of the total, accounts for approximately one-third of the births and one-half of the natural increase each year. The net reproduction index, where unity represents permanent replacement, in 1930 was only 0.86 for the urban population, but it was 1.37 for the rural non-farm and 1.69 for the rural farm population. By way of contrast it may be pointed out that 81 of the 93 cities in the United States with a population of more than 100,000 in 1930 had reproduction indexes of less than 1, while in the white rural farm population only 41 out of 2,982 counties [1] had indexes of less than 1, and 397 counties had indexes of more than 2.

Nevertheless, the number of people living on farms at the present time is approximately the same as in 1910, though the total population of the country has increased by 33 per cent since then. The proportion of the total population living on farms has decreased from one-half to one-fourth in less than a century. Since the intrinsic rate of growth of the farm population has been greater than that of non-farm population, this result was possible only through extensive migrations from farms to villages,

[1] Excluding counties with fewer than 100 white rural farm women, aged 20–44.

towns, and cities. Rapid technological changes have made it possible for a relatively stable number of farmers to produce the food and fibers for the growing nation and at the same time have opened up new occupations in producing, distributing, and servicing more and more industrial goods and producing more in the way of professional services. The increase in the productivity of the average worker in agriculture has kept pace with that of the average factory worker.[1]

With the growth of more efficient methods of production and the rationalization of the agricultural enterprise there has been a reorientation of agriculture away from a self-sufficing toward a commercial focus—from production primarily for home use to production for the market. Carried to its logical conclusion, commercial agriculture implies a point of view which looks upon the farm as a factory for converting the elements of the soil into food and fiber and which strives for that most productive combination of land, labor, capital, and management which will yield the largest net return. The degree to which this point of view has been accepted varies widely from the fruit and vegetable farms on the Pacific Coast to self-sufficing farms in the New England hills, from the cotton farms of the South to part-time farms near the Great Lakes. The increases in efficiency were not equally distributed in all parts of the country or to all agricultural groups. Variations in climate and soil, in type of farming, and in cultural traditions have made for a very uneven spread of the newer techniques and the thought patterns associated with them.

There are also wide differences in the rates of reproduction of the rural farm population, both white and Negro. For the native white group the rates of reproduction range from 1.00 in Connecticut to 2.11 in Utah and among Negroes they range from 1.47 in Arkansas to 2.14 in North

[1] Wallace, Henry A., *Technology, Corporations and the General Welfare.* University of North Carolina Press, 1937.

Carolina. In general, rates are higher in the South than in the North and West; among Negroes and other colored groups than among whites; among foreign than among native stocks. The inverse relationship between levels of living and fertility ratios applies also within the farm population. In general, where farming incomes and levels of living are low, rates of natural increase in the farm population are high and vice versa, but there are numerous exceptions. Some areas with high reproduction rates also have a high plane of living index and within the areas where a low plane of living and low farm incomes prevail there are considerable variations in reproduction rates. Some of the highest fertility ratios among whites are found in Utah where the farm plane of living index is relatively high, in sections of North Dakota where the plane of living index is intermediate, as well as in the Southern Appalachians where the plane of living index is very low. Rates among rural farm Negroes are generally high, but in a number of Southern States, notably Georgia, Tennessee, Kentucky, Alabama, Mississippi, Arkansas, Louisiana, and Oklahoma, reproduction rates for the native white farm population are greater than for the Negro rural farm population.[1] There are also considerable variations in the reproduction rates of the Negro farm population. Fertility ratios among Negroes, in the areas where plane of living indexes are typically low, range from less than 600 in the Yazoo Mississippi Delta to more than 1,000 in the tobacco growing areas of North Carolina and Virginia.

The variations among both white and colored groups suggest the influence of the character of the agriculture upon rates of population growth. A highly rationalized agriculture, utilizing modern techniques and oriented toward production for the market, is relatively inelastic for

[1] Warren S. Thompson. "Standardized Replacement Indices for Native-White and Negro Populations of Cities of Three Size Classes and of Rural-Nonfarm and Rural-Farm Communities, by States: 1930 and 1920." *Population Index*, October, 1938, 4, No. 4, pp. 267–276.

population growth and where it predominates reproduction rates tend to drop toward urban levels. In periods of severe economic stress such areas are incapable of absorbing any considerable increase in numbers and may even experience a net loss. In contrast there are in American agriculture extensive areas in which agriculture is not primarily oriented toward a market, but may be characterized as essentially non-commercial. In these areas rates of reproduction have been maintained at relatively high levels. These areas have shown a high degree of elasticity for population and during the years since 1930 have absorbed not only their own natural increase, but a large share of the migrants from urban areas as well.

No one factor can serve to account for the variations in reproduction rates in the farm population, for the pertinent social and economic factors are closely interrelated and it is difficult to isolate any one of them. Moreover, a satisfactory statistical measure of the extent to which commercial agriculture prevails has not been developed.[1]

Nevertheless, the relationships may be indicated by comparing fertility ratios for the native white farm population with several measures of the degree to which agriculture is oriented toward commercial production (Table 1). The highest reproduction rates are found in the Southern States, including the Southern Appalachians. These are the regions where capital and equipment per agricultural worker and the proportion of farms hiring labor is least and where the percentage of farms reporting gross incomes of less than $1,000 in 1929 is greatest. The East South Central and the South Atlantic Divisions include many of the smaller farms on which the major share of the product was used for home consumption, *i.e.*, farms which are essentially non-commercial. They also include the majority of cotton farms where production is centered

[1] See Taylor, Carl C., *Prospective Development of Cultural Patterns in Rural America and Their Possible Influence on Population Trends*, for a discussion of the meaning of the term "commercial agriculture."

TABLE 1

Fertility Ratios of White Rural Farm Population and Related Agricultural Factors, 1930

Geographical Divisions	Fertility ratios [1]		Livestock and machinery per worker [2]		Per cent of farms self-sufficing [4]		Per cent of farm products used by operator's family [3]		Per cent of farms reporting expenditures for labor [3]		Per cent of farms with gross incomes less than $1000 [3]	
	Ratio	Rank	Value	Rank	Per cent	Rank	Per cent	Rank	Per cent	Rank	Per cent	Rank
North Eastern.... . .	622	8	$1000	4	8.9	3	11.6	6	59.1	8	41.2	4
Middle Atlantic	649	7	1215	6.5	6.8	4	12.3	5	55.4	7	35.8	5
East North Central . .	670	6	1215	6.5	5.4	5	13.1	3	46.4	4	34.4	8
West North Central.. ..	696	5	1865	8	4.4	7	11.0	7	54.3	5	25.9	9
South Atlantic	839	2	475	2	13.4	2	19.8	2	34.7	2	65.0	2
East South Central .	858	1	320	1	13.9	1	22.2	1	23.5	1	71.3	1
West South Central . ..	803	3	520	3	4.7	6	13.0	4	35.8	3	59.3	3
Mountain.......... ..	787	4	2130	9	4.3	8	6.5	8	54.8	6	34.7	7
Pacific	541	9	1205	5	3.6	9	4.3	9	62.3	9	35.3	6
United States...	752	—	1175	—	7.9	—	12.7	—	41.8	—	48.8	—

[1] P. K. Whelpton, "Geographic and Economic Differentials in Fertility." *The Annals of The American Academy of Political and Social Science*, November 1936, pp. 37–55.

[2] John D. Black, "Agricultural Population in Relation to Agricultural Resources." *The Annals of the American Academy of Political and Social Science*, November 1936, pp. 205–217.

[3] United States Census 1930, Agriculture IV.

about a cash crop. But, although rural farm fertility ratios throughout the South are high, it is worthy of note that the lowest fertility ratios reported for the colored rural farm population are found in a strip along the lower Mississippi where the highest rates of tenancy in this country are found. In this area, more than in cotton growing areas farther east, production has been rationalized and there appears to have been some tendency to vary farm organization in accordance with the fluctuations in the income from cotton.

Such a comparison may also be made between fertility ratios and the per cent of farms with gross farm incomes of less than $1,000 in 1929. So far as this measure can be used as an index of agricultural income, it would appear to bear out the general result of studies in differential fertility which show an inverse relationship between incomes and reproduction rates. However, this percentage may also be used as indicating the presence of non-commercial farms, for, in effect, the farms with gross incomes of less than $1,000 in 1929 were non-commercial—the 49 per cent of all farms which in 1929 reported gross incomes of less than $1,000 sold only 11 per cent of the marketed crops.

Whether greater emphasis should be given to plane of living than to the character of the agriculture is perhaps a matter of choice. Within the present organization of American agriculture non-commercial farming is likely to be associated with low incomes and low plane of living, but commercial agriculture does not assure a high plane of living for white or colored farmers. However, in view of the apparent elasticity of non-commercial farming areas for population, it seems proper to attribute some significance in explaining fertility ratios to the character of the agriculture as more or less oriented toward a market.

Within areas characterized by the same plane of living considerable differences in fertility ratios are found. In order to show the relationships more clearly the counties for the United States were classified according to a rural

farm plane of living index based on the possession of certain facilities and the value of the farm dwellings. The lowest fifth of the counties was selected for further analysis (Table 2). Most of the 627 counties involved are located in the Southern States. If they are further subclassified according to the proportion of the products used by the operator's family, it will be seen that for both whites and negroes the fertility ratios increase as the proportion of farm products consumed by the operator's family increases. In other words, at this low material plane of living the fertility ratios decrease as the degree to which agriculture is commercialized increases. In this same group of counties the fertility ratios for both whites and negroes decrease as the proportion of farms operated by tenants increases.

Although the plane of living index in the cotton belt is somewhat below that in the Appalachian-Ozarks, where much of the farming is of a self-sufficing, non-commercial character, the fertility ratios in the cotton belt are considerably below those in the Appalachian-Ozark Areas, 757 compared with 842. Moreover, the lower rate is based on a population which includes a large proportion of negroes and the higher rate is observed in a population with almost no negroes. This is entirely consistent with the observation that fertility ratios for the native white farm population of the East South Central States are almost as great as those for the negro population of the South Atlantic States. In other words, in these two areas characterized generally by high fertility ratios the differences between commercial and non-commercial farming transcend the differences between the races.

Differences in the volume and direction of changes in the farm population in recent years may also be explained in part by the character of the agriculture. The capacity of certain agricultural areas to absorb additional population is illustrated by the movement of farm population between 1930 and 1935, a period when migration from farms

TABLE 2

RURAL FARM FERTILITY RATIOS AND PLANE OF LIVING, PER CENT OF FARM PRODUCTS USED BY OPERATOR'S FAMILY, PER CENT OF FARMS OPERATED BY TENANTS, AND PER CENT OF FARMS REPORTING EXPENDITURE FOR HIRED LABOR, 1930[1]

Item	Number of counties	Median fertility ratio white rural farm
Farm Plane of Living Index 1–30		
Total	627	842
Per cent of Farm Products Used by Operator's Family		
0–16.9	172	830
17–21.9	161	804
22–30.9	145	836
31 and over	149	909
Per cent Tenancy		
0–39.9	150	899
40–59.9	153	856
60–72.9	158	808
73 and over	166	794
Per cent Reporting Expenditure for Hired Labor		
0–19.9	150	846
20–25.9	175	828
26–32.9	150	845
33 and over	152	851
Farm Plane of Living Index 31–70		
Total	622	772
Per cent of Farm Products Used by Operator's Family		
0–11	156	795
12–19	144	755
20–26	157[2]	758
27–67	164	790
Per cent Tenancy		
0–23	158[2]	784
24–37	161	793
38–57	150	768
58–84	152	757

[1] Data from U. S. Census, 1930.
[2] Data not available for one county.

TABLE 2—(*Continued*)

Item	Number of counties	Median fertility ratio white rural farm
Per cent Reporting Expenditure for Hired Labor [2]		
0– 26	157	777
27– 32	149	767
33– 45	161	760
46–154	154	785
Farm Plane of Living Index 71–120		
Total	680	703
Per cent of Farm Products Used by Operator's Family		
0 – 7.4	158 [2]	709
7.5–12.9	178	734
13 –18	164	676
19 –38.9	179	719
Per cent Tenancy		
0–16	176	728
17–27	169	684
28–39	164	703
40–80	171	681
Per cent Reporting Expenditure for Hired Labor		
0–38	173	660
39–47	162	675
48–59	185	721
60–93	160	744
Farm Plane of Living Index 121–170		
Total	636	644
Per cent of Farm Products Used by Operator's Family		
0– 7	150	658
8–10	145	645
11–13	173	619
14–30	168	648
Per cent Tenancy		
0–19	173	645
20–34	159	615
35–44	157	632
45–69	147	665

TABLE 2—(*Continued*)

Item	Number of counties	Median fertility ratio white rural farm
Per cent Reporting Expenditure for Hired Labor		
0–46	161	600
47–56	165	626
57–65	152	652
66–86	158	689
Farm Plane of Living Index 171 +		
Total	493	569
Per cent of Farm Products Used by Operator's Family [2]		
0 – 6	112	555
7 – 9.4	129	587
9.5–11.9	120	574
12.0–45	131	572
Per cent Tenancy		
0 –12.4	127	540
12.5–24	129	546
25 –44	121	573
45 –69.5	116	614
Per cent Reporting Expenditure for Hired Labor		
0–53	126	559
54–59	116	554
60–65	124	582
66–92 9	127	586

generally was radically slowed down and there was some movement from urban to rural areas including farms. (Movement in the Great Plains areas and the Pacific Coast States cannot be considered here because of the effect of severe and protracted droughts.) During that period, when farm population generally was increased, several States reported a loss in farm population. These were Iowa, Mississippi, Georgia, and California—States in which agriculture is predominantly commercial. Although Iowa's farms are relatively productive and therefore might be presumed to be able to absorb a substantial increase in

farm population, they did not do so. In fact, the net migration from Iowa farms during the 5 year period was nearly 12,000 per year, compared with nearly 16,000 per year between 1920 and 1930. Throughout a large section of the Cotton Belt the net migration between 1930 and 1935 was from farms to towns and cities, as it had been during the decade before 1930. Negroes who had comprised about one-third of the migrants from farms between 1920 and 1930, and who are found primarily in areas where commercial agriculture predominates, appear to have been underrepresented in the movement to the land which occurred after 1930. Those areas which had most definitely experienced the effects of decreased demand for man-power in agriculture before 1930 appeared less able to absorb either their own natural increase or the movement back to the land during depression years, but in those areas where agriculture is directed more toward family subsistence, where self-sufficing and part-time farming predominated, there was a definite tendency to retain a large proportion of the natural increase and to receive many of the migrants from towns and cities who turned to agriculture. The possibility of procuring subsistence for themselves and their families might have been better in those areas in which commercial agricultnre has been highly developed, but there the reorganization which would have been required if large numbers had been added did not occur. In fact, much of the increase in the farm population since 1930 went to augment that part of the farm population which even in 1929 prodnced almost none of the marketed agricultural commodities.

Although it appears that migrations to and from farms between 1930 and 1935 can be accounted for to a large extent by the degree to which agricultnre is oriented toward the market, no similar explanation can be given for migrations to and from farms between 1920 and 1930,[1] the

[1] Goodrich, Carter, et al. *Migration and Economic Opportunity.* University of Pennsylvania Press, Philadelphia, 1936. pp. xvii + 763.

only other period for which detailed figures are available. The development of effective urban opportunities, expansion and contraction of agriculture, and a wide variety of employment opportunities make it difficult to account for the migrations to and from farms through the use of any one set of factors. Some of the greatest rates of migration from farms were reported from areas where commercial agriculture is dominant, but areas where agriculture was chiefly non-commercial also experienced a large net migration from farms. The migration during that period was predominantly away from farms and, therefore, an analysis of the "pull" factors would appear to be necessary for a complete explanation of the variations found.

It need hardly be emphasized that the factors related to population growth are numerous and complex. For those areas where plane of living is lowest, relationships between the degree to which agriculture is commercial and reproduction rates have been shown. For other areas or groups of counties similar relationships are less clear or non-existent. Whether more refined data would eliminate these difficulties remains an open question. It may be pointed out, however, that in the areas characterized by low plane of living there may be less differentiation among the several classes of the farm population, operating owners, tenants, and laborers, than in those areas where levels of living are higher. A number of studies have shown that in some areas fertility ratios are higher among farm laborers and tenants than among owners.[1] In such areas the proportion of tenancy may be a measure of the commercialization of agriculture, but in the absence of refined rates the greater fertility among tenants may obscure other relationships (Table 2).

Nevertheless, the character of the agriculture appears

[1] Notestein, Frank W., "The Differential Rate of Increase Among the Social Classes of the American Population." *Social Forces*, Oct., 1933, 12 (1): 17–33. See also Lorimer, Frank, and Osborn, Frederick, *Dynamics of Population*. The Macmillan Company, New York, 1934, pp. 84–92.

to provide a useful clue in accounting for differentials in population growth in the farm population. The attitudes toward farming which make for a commercial agriculture are part of a larger complex of attitudes which are in turn part of the modern industrial world and the extent to which they are found can be taken as an index of the prevalence of other related attitudes and the extent to which those which are inconsistent have been altered, submerged, or discarded. Attitudes regarding family size are not likely to remain static in a world in which changes are being made in other spheres of behavior. Large families were part of the culture complex which included a predominantly self-sufficient agriculture; smaller families appear to be part of the culture complex which includes a predominantly commercial agriculture. If this be correct it may serve in part to explain some of the variations within the farm population itself, as well as the typical differences between farm and non-farm populations.

THE SOCIAL ENVIRONMENT AS A FACTOR IN POPULATION GROWTH

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(Read November 18, 1938, in Symposium on Population Growth)

THAT the social environment is a factor of prime importance in determining population growth need not be argued today in this country. The recent studies of Pearl and Notestein and Stix have shown beyond question that voluntary control of conception in the United States is so effective that it will account for large differences in the birth rates of those who do and do not practice it. The question most needing investigation now is why voluntary control of conception is practiced in such varying degrees by different groups in the population. Since no positive answer can be given to this question at the present time, all that a discussion of this subject can do is to call attention to some of the social situations which seem to have more or less effect upon the desire of people for offspring and which presumably do or would affect the practice of contraception by individuals.

When any community develops a set of mores or habits of thought which encourage or discourage people in the raising of families, it may truly be said that the social environment is an important factor in population growth. Thus in China the entire life of the individual is very greatly influenced by what is considered to be for the good of the family. The individual acquires his significance as a member of a family rather than as an individual. From their very birth children are trained to become functioning members of a family group. Neighborhood groups, play groups, economic groups, and political groups are all of

secondary importance. The first and chief responsibility of every one is to his family. Under these circumstances the continuance of the family is one of the most important functions of each member. It is not difficult to understand, therefore, that the raising of children for the continuance of the family is considered the first duty of practically every adult.

I do not mean to imply that these Chinese mores always result in the highest possible birth rate, nor that they would absolutely forbid the practice of contraception. But I do believe that where there is such a strong public sentiment encouraging people to raise families and where one's position in the community is based so largely upon his family status, such a set of mores would have a marked effect upon the proportion of adults marrying and upon the number of births in the family, even if means of contraception were generally known and easily accessible. I do not see how anyone can seriously question this position.

To bring this matter of the effect of the social environment—the climate of opinion—on population growth somewhat nearer home it will be of interest to note the effects of the Nazi propaganda for a higher birth rate among the Germans. Since 1933 the Nazi government has been putting forth great efforts to change the German birth rate through changing the mental attitudes of the people towards the raising of larger families. There has been no question of changing the biological or physiological capacity for reproduction. It was assumed from the start that the low birth rate of Germany was the consequence of the voluntary action of the people arising from their lack of faith in their future. The Nazis said that this was the natural consequence of the breaking of the German spirit by the loss of the war and the harsh conditions imposed upon them by the peace treaty. It was assumed that the revival of self-confidence in the people and the opening up of an attractive future for Germany would lead to increased readiness to marry and to the raising of

larger families. But encouragement of larger families was not left solely to vague and general political and economic measures. Marriage loans largely repayable with children were established to enable young people to marry early, large families were subsidized, rural living was encouraged, the means for practicing contraception were made difficult to secure, and the practice of abortion was punished certainly and severely. The Nazis estimate the total effect of their drive for more births has already added about 1,170,000 births to those which would have occurred had the low level of 1933 continued through 1937. The high officials of the party attribute most of this increase to the revival of the German spirit, to the restoration of the faith of their people in their future so that they are now willing to raise larger families than in the days before 1933 when their government could promise them no attractive future.

One who tries to keep his feet on the ground will want to compare the increase in marriages following 1933 with that in other countries and will connect the increase in births with the more vigorous enforcement of the laws forbidding abortion. But even when allowance is made for these factors there is a goodly increase in births in Germany which seems to be more or less closely connected with the changes in the climate of opinion which followed the accession to power of the Nazis. Only the future will tell whether the Nazi ideals and policies will prove effective in raising births over a long period, but this brief experiment does seem to indicate that the influence of social environment in population growth is a very real thing; indeed, it suggests that it may be the decisive factor in determining the birth rate in modern communities.

In the United States we can also point to many facts which support this conclusion. Pearl and Notestein and Stix, whose work has already been mentioned, have shown beyond doubt that in this country, voluntary control of conception is highly effective and accounts for a large part of the difference in the birth rates of those who do and

those who do not practice contraception. Furthermore the differences in mores and habits of thought between communities having high birth rates and those having low birth rates would of itself lead us to assume that these differences are of prime importance. Almost without exception the areas of high birth rates are more or less isolated areas into which ideas from the outside world penetrate slowly. Here the older mores approving early marriage and large families still persist. Here, too, as in the case of the Mormons in Utah, religion reenforces these mores and strengthens the community approval of large families. Even within an urban community the groups having the largest families contain those people who are most isolated from the more recent currents of thought regarding the *sphere* of woman, the *place* of the family, the *right* of the individual to live his own life, and other modern ideas which are apt to lead to the disintegration of the nineteenth century concepts of family life.

In proof of this position it may be mentioned that the replacement index for 1930 (the rate at which the present population would reproduce if specific birth rates and death rates remained as they were in 1930 and there were no migrants into or out of the community) for the farm population in West Virginia, North Carolina, and Alabama (the highest ranking states) was over 2.00, that is, the farm population in these states would slightly more than double in a generation. At the other extreme we find that the replacement index in the cities of over 100,000 in California was only 0.55, slightly more than half enough to replace present numbers. In the United States as a whole the farm population had a replacement index of 1.69 while the cities of over 100,000 had an index of only 0.76. Why these large differences?

There is certainly no evidence that biological or physiological differences will account for any considerable part of these large variations in rates of reproduction. On the other hand, as said above, recent studies indicate that the

low rates are voluntary, that is, that they arise largely out of the different attitudes of mind among the people in the low birth rate groups from those prevailing among the high birth rate groups. I would hasten to modify this statement by recognizing that even if there were a strong desire on the part of the high birth rate groups to lower their birth rates it would encounter very great difficulties in making itself effective since the knowledge of, and the opportunity to practice contraception are very much restricted in those groups. But, of course, it will be recognized at once that this knowledge and opportunity are themselves determined by the social environment rather than by any natural biological or physiological limitations of the people concerned.

I have dwelt at considerable length on the decisive importance of the social factors in population growth not because this particular group is skeptical on this point but because those who are less well-informed are quite likely to be confused. This might well prove a serious matter if, as is not unlikely, there should be a strong movement to do something to increase the birth rate when it becomes generally known that we are about at the end of our natural population growth. The point I would like to make here is that if we should ever decide that we want to control population growth we can do so if we know how to construct the proper kind of environment, if we develop the right kind of social organization. Manifestly it will have to be a social organization in which the climate of opinion leads all normal people to want to participate in the future through their children and by which they are informed of the extent it is desirable so to participate. It will have to be an environment in which there is no handicapping economic or social discrimination between those who want to contribute to community life by raising families of the proper size as well as through their own work, and those who are interested in making their contribution

only through their own work. I realize that such a vague general statement is of little value, but more cannot be said in the present state of our knowledge of the motivation of the birth rate. We do not know why almost 30 per cent of our women have no children, 18 per cent have only one child and another 18 per cent have only two children. We are prone to assume that the economic motive is the chief one—that the difficulty of making ends meet leads to the curtailment of births. But all our information shows that the fewest children are found in the most prosperous classes. Thus in the eight cities for which census tract data are available there is a direct relation between the payment of high rents and a small number of children. Thus in Boston the fourth of the tracts where rents are lowest (averaging from $19.10 to $28.60) has 450 children per 1,000 white women 15–44 years of age, while the quarter of the tracts having the highest rentals (averaging from $48.50 to $155.90) has only 271 children to the same number of women. In the other cities the figures are somewhat different but the general situation is the same. In no city are the ratios of children to women in the poorest quarter of the tracts, as judged by the average month rental, less than 45 per cent higher than in the most prosperous quarter and the average is about 75 per cent. If the proportion of the gainfully employed workers engaged in manufacturing in these tracts is used as a measure of economic and social status then the larger the proportion of such workers the higher the ratio of children to women. In no case does the quarter of the tracts having the highest proportion of workers engaged in manufacturing have less than 53 per cent more children per 1,000 women than the quarter in which such workers are fewest. Again the average number of children is about 75 per cent greater in the tracts of low social and economic status.

These and other facts abundantly indicate that if the economic motive is the dominant motive in family restriction it should not be confused with the poverty motive.

Indeed a so-called economic motive which leads the well-to-do to restrict the size of their families much more drastically than the poor must be compounded of a variety of motives which have little connection with actual economic necessity. This fact needs to be recognized and this so-called economic motive must be analyzed into its component parts before we shall be able to understand why we have our present differentials in birth rates.

It seems clear in the light of these facts that the common statement that "we cannot afford more children" cannot be accepted as an adequate explanation of the small families which are so common among the comfortable and the well-to-do. The fact is that such a statement—"we can't afford more children"—is far from accurate or at least it is incomplete. What it really means is that if we have more children we cannot travel as we would like to, we cannot live in as good a house as we want to, we cannot drive as good a car as our neighbors, we cannot go out (theater, bridge, etc.) as much as our friends, I shall not be able to go with my husband as frequently as otherwise, etc., etc., ad infinitum. In other words the whole scheme of life and scale of values we have come to regard as valid has no place in it for the three or four children needed to maintain numbers. In many sections of the population children simply are not among the more important values of life. Why is this so? Why have mental attitudes hostile to reproduction become so widespread? Why are they more operative in certain groups than in other groups?

We students of population must acknowledge that up to the present we have been so busy finding out where these differentials existed, and how great they were, and the effects of the decline of the birth rate on the future growth of population, that we have given little attention to how these declines have come about, to the *why* of the change. Clearly if life is worth while at all, if our civilization is worth perpetuating, there must be children to carry on.

There may not need to be enough to support 150 millions of us, but a decline of a fourth in a generation, as in our larger cities in 1930, can scarcely be regarded with entire equanimity. If we think that a stabilized population is desirable and if we look forward to its achievement, then we, as students, should begin soon to study the *why* of the decline of the birth rate below replacement level so that we shall know how to change the social environment to control numbers if, and when, such a control becomes a matter of public policy.

INTRINSIC FACTORS IN POPULATION GROWTH [1]

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(Read November 18, 1938, in Symposium on Population Growth)

It is not the purpose of this paper to examine the influence of fundamental biological or social changes on population growth. Instead, its purpose is to consider some of the more important direct factors through which such fundamental changes must operate to modify population growth. These direct or immediate factors may be thought of as causal only in a limited sense. They are better described as the means through which changes occur. The usefulness of such an approach to the problem of population growth lies in the fact that it narrows the field of idle speculation and points the direction in which the search for fundamental causes should proceed. The present paper, which must suggest rather than treat the problem, will devote brief attention to the influence of changes in the age distribution of the population and of changes in mortality and then proceed to somewhat more detailed examination of elements of variation in fertility.

The age composition of any population is determined by the birth rates, the death rates, and the migrations of the past. In the United States, and indeed in most Western countries, these past events have generated a present population in which the ages of low mortality and high fertility are heavily represented. The result is a population favorably constituted to yield a natural increase. This situation is transitory, and the transition will greatly modify our vital rates. An example, utilizing the tech-

[1] From the School of Public Affairs, Princeton University, in cooperation with the Milbank Memorial Fund.

nique developed by Dublin and Lotka, will serve to make the matter clear. In 1936 the crude birth rate for the white population was over 17 [1] and the crude death rate was under 12, leaving a natural increase of more than 5 per 1,000 population. If the same fertility and mortality schedules that yielded these rates in the 1936 population were to remain in force until they generated their own characteristic age distribution, they would yield quite different vital rates. The birth rate would then be 14.5 and the death rate 16.4, yielding a natural decrease of 2.0 per 1,000 inhabitants.[2] Today's actual increase is the result of a favorable age composition inherited from the past. The age distribution of the future, now being generated, will be much less favorable to natural increase.

Population growth has been greatly stimulated by the dramatic reduction of mortality in recent times. In 1901 the expectation of life at birth was 49 years; by 1936 it had increased to 61 years.[3] The importance to population growth of such an increase in life expectancy may be seen from its effect on the birth rate required to maintain a stationary population. In 1901 the required birth rate was 20.3; in 1936 it was 16.4 or nearly one-fifth lower. Such gains cannot continue indefinitely short of wholly unforeseen developments. Dublin and Lotka have shown that there are no present indications that the expectation of life will rise to much above 70 years.[4] Such a development would only reduce the birth rate required to maintain a stationary population from 16.4 to 14.3.

Recently, declines in fertility have been much more

[1] This estimate makes allowance for under-registration of births.

[2] *Population Index*, iv, No. 4, October, 1938, p. 265. The intrinsic rate of natural increase relates to the total white population, but the birth and death rates relate to the female population.

[3] *Statistical Bulletin*. Metropolitan Life Insurance Company, xix, No. 8, August, 1938, p. 6. The figure for 1901 is for the Original Death Registration States. That for the whole country in 1901 would have been somewhat lower.

[4] Dublin, Louis I., and Lotka, Alfred J. *Length of Life*. The Ronald Press Company, New York, 1936, p. 194.

rapid than those in mortality. The gross reproduction rate for 1935 was lower than the net reproduction rate for 1930.[1] In other words the effect on net reproductivity of the decline in fertility between 1930 and 1935 would not have been fully counterbalanced by the elimination of all deaths between birth and the end of the childbearing period.

Under modern conditions of low mortality and severely restricted foreign immigration, fertility is much the most important variable in population growth. Falling birth rates are by no means new to this country as may be seen from Fig. 1. The decline has been virtually continuous

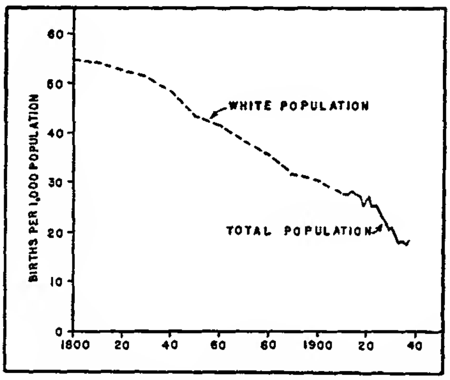

FIG. 1. Trend of the birth rate in the United States: 1800–1937. (Estimates for 1800–1937 from Thompson and Whelpton, *Population Trends in the United States*; those for 1934–1937 are official rates adjusted for under-registration of births.)

since 1800 and has gone on with increasing speed since 1925. There has been a slight check in the last few years; but, following a precipitous drop during the early years of the depression, it does not suggest more than a transitory change.

There are, of course, large differences in the fertility of constituent groups of our population. Some of the poorest agricultural groups are contributing more than twice their

[1] *Population Index*, iv, No. 2, April, 1938, p. 127.

own numbers to the next generation.[1] On the other hand the white-collar groups in some urban communities are contributing less than half their own numbers to the next generation.[2] In this paper it will be possible to consider only briefly the general patterns into which the differences fall and to illustrate them by material for the East North Central States from the unpublished sixth count of the family card of the 1930 federal census. Actually the data do not relate directly to fertility. Instead they give the average number of children under age 10 living at home per wife for marriages of 5 to 9 years' duration in 1930.[3] The families considered are those in which the husband was the household head and the wife the homemaker and neither spouse had been married before. Elsewhere the writer has shown that such data yield a fairly reliable picture of the patterns of differential fertility.[4]

For present purposes the most important patterns in differential fertility may be generalized into four types of associations.

1. Fertility is inversely associated with social-economic status. The relation is illustrated in Fig. 2 which utilizes value of home as an index of economic status. In each type of community and each color-nativity group the number of children tends to decrease with value of home. Classifications based on income or occupational status yield analogous patterns.

One major exception to the inverse relation appears. Throughout the native-white population and in the foreign-born population of large cities the fertility of the highest

[1] National Resources Committee. Committee on Population Problems. *Problems of a Changing Population.* United States Government Printing Office, Washington, 1938, pp. 122–123.

[2] The estimated net reproduction rates of many communities are below 0.60; those in certain upper occupational classes must drop below 0.50.

[3] Any child related or connected to the husband or wife was included in the census family.

[4] Notestein, Frank W. "Differential Fertility in the East North Central States." *The Milbank Memorial Fund Quarterly*, xvi, No. 2, April, 1938, pp. 173–191.

value-rent group exceeded that of the next highest group. Other data have indicated similar exceptions. Whelpton has shown that in the well-to-do districts of certain cities fertility tended to rise with median rental.[1] Kiser, utilizing data for five cities from the National Health Inventory,

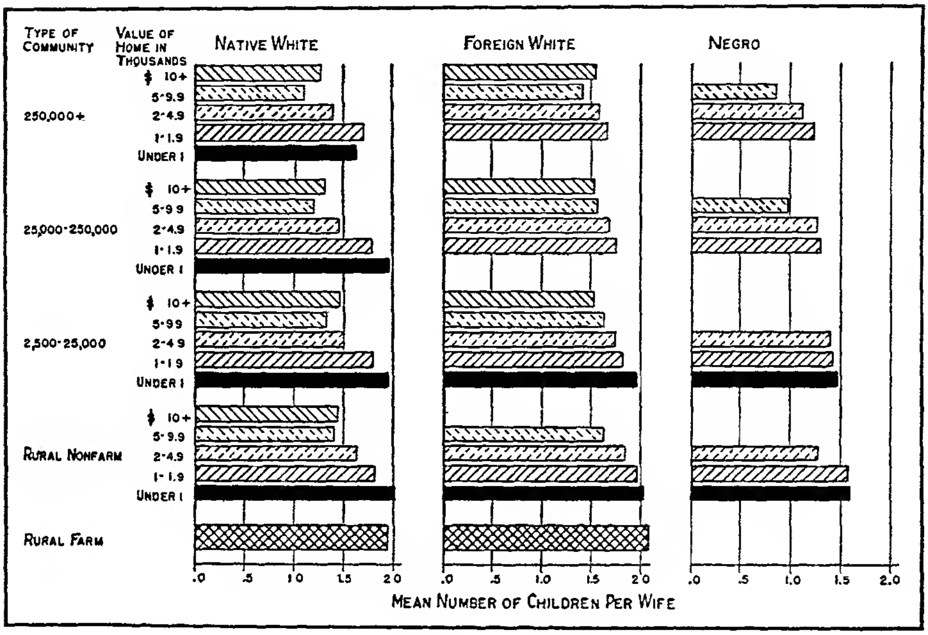

FIG. 2. Relation of value of home to mean number of children under age 10 per wife, for marriages of five to nine years' duration, classified by color-nativity and type of community: East North Central States, 1930. (Classes with less than 200 cases omitted.)

has shown that the annual marital fertility rate was higher in the professional class than in the business class and higher for families with annual incomes in excess of $3,000 than for those with incomes of between $2,000 and $3,000.[2]

[1] Whelpton, P. K. "Geographic and Economic Differentials in Fertility." *The Annals of the American Academy of Political and Social Science,* 188, November, 1936, pp. 37–55.

[2] Kiser, Clyde V. "Variations in Birth Rates According to Occupational Status, Family Income, and Educational Attainment." *The Milbank Memorial Fund Quarterly,* xvi, No. 1, January, 1938, pp. 38–56.

The interpretation to be given these exceptions to the familiar pattern is by no means clear, but it is tentatively suggested that they represent the beginning of a reversal in the standard inverse association of fertility and economic status.

2. Fertility is highest in the rural population and declines as the size of community increases. Fig. 3 shows that this relation holds for each color-nativity group.

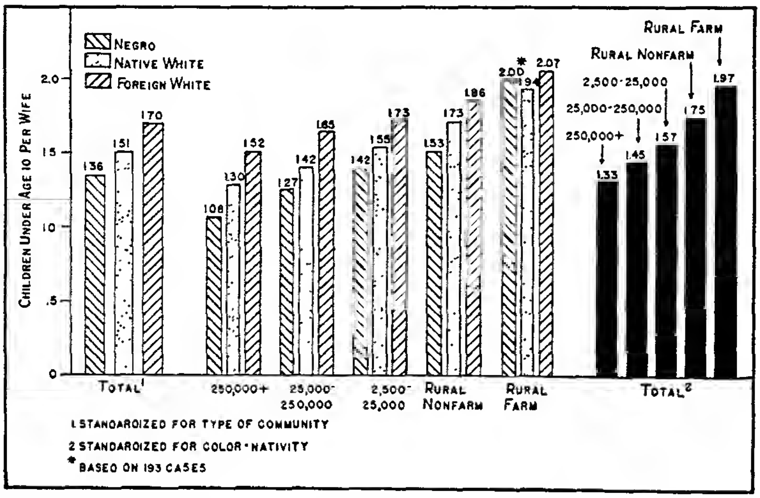

FIG. 3. Relation of color-nativity and type of community to the mean number of children under age 10 per wife, for marriages of five to nine years' duration: East North Central States, 1930.

3. The foreign-born population is more fertile than the native-white population, and Negroes are less fertile than the native-white population. These relations hold for each type of community with one exception. Negroes appear to be more fertile than native whites in the farm population. The present sample is too small to yield reliable results on this point, but in the South, the only place where the relation is important, rural Negroes definitely are more fertile than rural native whites.

4. Regional differentials in fertility are large and im-

portant. The country is recruiting its population heavily from the poorer agricultural regions of the South and West. Basically, however, these differentials appear to arise chiefly from combinations of the foregoing associations. For example, the net reproduction rate of the Southern population is much higher than that of most other regions, but the economic status of a large proportion of its population is low. Within comparable plane-of-living groups, the Southern population appears to be no more fertile than that of other regions.[1]

Differences in age at marriage influence differences in fertility to some extent, but they are neither highly important nor do they consistently tend to build up the general patterns just described. For example, the average age at marriage is lower for Negroes than for native whites, but Negroes are the less fertile. On the other hand, the foreign-born population marries later than the native-white but in spite of that fact is more fertile. In the case of the social-economic and type-of-community groups, differences in marriage age do contribute to the general fertility pattern. Late marriage is probably a substantial factor in the relatively low fertility of certain professional and business groups.

Even if there were no differences in age at marriage, differences in fertility would be substantially of the same sort as those already observed. This fact is shown by the two left-hand panels of Fig. 4. Fertility declines sharply with age at marriage, but in virtually every age-at-marriage group it is inversely associated with size of community and is lowest for Negroes and highest for foreign born. Further classifications of the data, which it is not possible to discuss here, indicate that, in general, the associations are much stronger for early than for late marriages.

Thus far differences in fertility have been discussed in terms of the average number of children per wife. This

[1] National Resources Committee, *op. cit.*, p. 136.

average conceals important relationships. Particular interest attaches to the proportion of wives that are childless. There are at least *a priori* reasons to suppose that childlessness is less a matter of individual choice than the

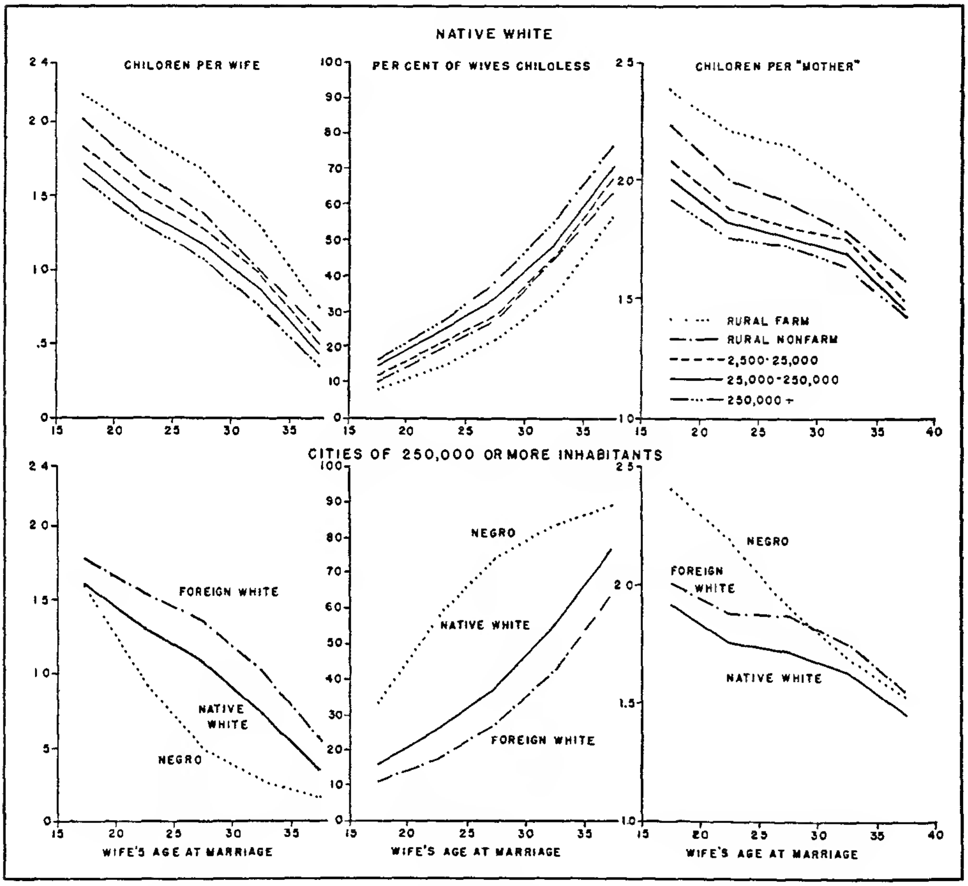

FIG. 4. Relation of age at marriage to the mean number of children under age 10 per wife and per "mother," and to the per cent of wives childless, for marriages of five to nine years' duration in selected type-of-community and color-nativity groups: East North Central States, 1930.

number of children in families with at least one child. The proportions are shown in the middle panels of Fig. 4. It must be remembered that they are the proportions of wives

married from 5 to 9 years who have no children living at home. They do not show the proportions of wives who had never borne a child but are largely controlled by those proportions. Three matters are worthy of special note.

1. The proportion of childless wives rises strikingly with advancing age at marriage.

2. Differences in the proportion of childless families are consistent with the general patterns of fertility already described.

3. The proportions are startlingly high in the case of Negroes. In the large cities in the East North Central States 53 per cent of the Negro wives married 5 to 9 years had no living children at home. The ratio was twice as high as that for native whites.

The low fertility of the Negro marriages was entirely due to the high proportion of childlessness. This fact is clearly shown by the lower right panel of Fig. 4, which gives, instead of the average number of children per wife, the average number of children per "mother," or, more precisely, per wife in whose home there was one child or more under age 10.[1] Negro "mothers" were not the least fertile of the color-nativity classes but the most fertile. Their average number of children exceeded that of the native-white "mothers" at each age of marriage and that of foreign-born "mothers" in the ages at which most marriages take place. The relation shown here for large cities also appeared in each of the other types of communities. Large proportions of Negro wives were childless, but Negro "mothers" were highly fertile.

Except for the high fertility of Negro "mothers" differences in the average number of children per "mother" contributed to the previously outlined pattern of fertility. The upper right-hand panel of the chart illustrates the relation for native-white "mothers" in various types of

[1] Not all of the children included were born by the wife. See note 3, p. 502.

communities. The number of children increased sharply with decreasing size of community. Further analysis of the data, which it is not possible to discuss here, indicates that, in the white population, variation with social-economic characteristics is larger and more consistently maintained in the average number of children per "mother" than in the proportion of childless marriages. This fact gives some support to the view that childlessness is less a matter of choice than the size of family with at least one child.

There is no reason to suppose that biological differences account for a substantial part of the group differences in the average number of children per "mother." Much the most important data on the subject are those obtained by Pearl from the reproductive record and contraceptive experience of about 30,000 confinement cases in Eastern urban hospitals. A preliminary analysis of the data for nearly 5,000 case histories of this series showed that, among women who had never practiced any form of contraception, there were no significant differences in the pregnancy rates of economic groups ranging from poor to well-to-do and rich.[1]

Results generally consistent with Pearl's findings have been obtained by Dr. Stix and the writer from an intensive study of the histories of nearly 1,000 patients of the Birth Control Clinical Research Bureau. The pertinent results are summarized in Fig. 5, which relates to the experience of the patients prior to their first visit to the birth control clinic. The figure shows pregnancy rates for experience when contraception was practiced and when no contraception was practiced for women classified by occupation of the husband and by religious affiliation. The rates are in the form of the number of pregnancies per 100 aggregate

[1] Pearl, Raymond. "Contraception and Fertility in 4,945 Married Women." *Human Biology*, vi, No. 2, May, 1934, p. 390.

person-years during which it was presumably possible to become pregnant.[1]

The main points of interest may be summarized as follows:

1. In each group the rates for contraceptive experience were much lower than those for noncontraceptive experi-

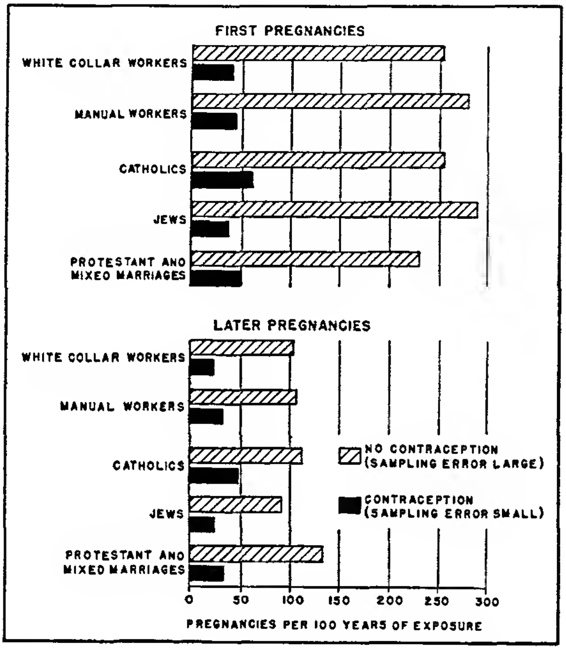

FIG. 5. Pregnancy rates for the contraceptive and noncontraceptive experience of married couples classified by occupational status and religion. (From pre-clinic experience of patients of a birth control clinic in New York City.)

ence. Although the women had received no clinic instruction the average risk of pregnancy when contraception was practiced was only about twenty-five per cent of that when contraception was not practiced.

[1] For a further description of the data and procedure see: Stix, Regine K., and Notestein, Frank W. "The Effectiveness of Birth Control. A Second Study." *The Milbank Memorial Fund Quarterly*, xiii, No. 2, April, 1935, pp. 162–178.

2. Few of the differences of the rates for noncontraceptive experience were statistically significant.[1] In a larger sample significant differences might emerge, but present evidence makes it abundantly clear that they would be inconsequential compared with those between the contraceptive and noncontraceptive experience of any group.

3. The differences of the rates for contraceptive experience were significant for the most part and, although rather small, run in the same direction as those in the average number of children borne.[2]

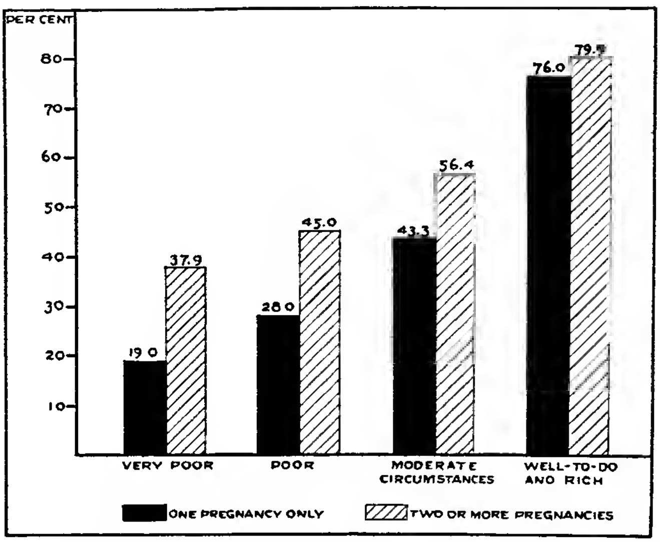

FIG. 6. Relation of the prevalence of contraception to economic status. (From Pearl, Raymond: "Contraception in 4,945 Married Women." *Human Biology*, vi, No. 2, May, 1934, p. 378.)

Differences in the prevalence of contraception are much more important than those in effectiveness. As Fig. 6 shows, Pearl found that the proportion of women practic-

[1] The differences in the rates for Jews and for the residual group comprising Protestant and mixed marriages are both significant but run in opposite directions. None of the other differences are significant.

[2] All differences are significant except those in the first pregnancy experience between white-collar and manual workers and between Catholic and Protestant and mixed marriages.

ing contraception varied from 19 per cent among the very poor to 76 per cent among the well-to-do and rich in the case of primiparæ and from 38 per cent to 80 per cent among multiparæ. The records of clinic patients prior to clinic attendance also indicate that contraceptive practice is more common among white-collar than manual workers and for the religious groups was most common among Jews and least common among Catholics.

Present evidence, admittedly incomplete, shows no substantial group differences in the pregnancy rates of overtly fertile women in the absence of contraception. It shows further that contraception greatly reduces the risk of pregnancy and that differences in its use are large and consistent with the general pattern of fertility. Even such fragmentary evidence points clearly to contraception as the primary means through which differences in fertility arise.

To view contraception as the means through which differences arise is not to say that contraception accounts in any fundamental sense for those differences. Our study of clinic patients shows that quite a high degree of effectiveness attaches to methods that utilize neither modern knowledge nor modern appliances. One such method, which has been widely known for thousands of years, yielded pregnancy rates 72 per cent lower than those for noncontraceptive experience. Some populations have utilized such common knowledge extensively and developed improved techniques. Others have scarcely utilized the knowledge at hand. The fundamental explanation of such contrasting behavior, and of the resulting differences and trends in fertility, must be sought in the different interests, values, and motives of a changing culture. It is to these matters that attention must be turned if we are to predict and control population growth.

A STUDY OF PSYCHOLOGICAL FACTORS RELATED TO FERTILITY

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(*Read November 18, 1938, in Symposium on Population Growth*)

The studies on which the data here reported are based were made under the auspices of the Pioneer Fund. I should like to acknowledge my special indebtedness to Mr. Frederick Osborn, the secretary of the board, for his assistance throughout this investigation. This discussion is intended merely as a preliminary report and will be confined to a presentation of certain typical raw data which have been obtained. Interpretations of these data will be reserved for a later date, since interpretations are likely to change as many items of information are woven together to give a more complete picture.

Probably the chief difference between this and most previous investigations in the field is that the present study is an intensive analysis of relatively small groups, whereas the usual study has concerned a limited number of factors in large populations. Well over a thousand separate items of information from each of 400 men and 300 women were collected in the present investigation. However, this study differs from most previous studies not only in the amount but also in the type of information secured from each individual. While most studies have been confined to the collection, tabulation, and analysis of objective data such as age, race, education, religion, occupation, income, size of community, age at marriage, etc., the present investigation also includes many subjective or psychological factors such as attitudes, interests, preferences, and values.

It does not seem necessary to insist that these psychological factors are of great importance in an investigation of such a topic as fertility. The chief question concerns the validity and accuracy of the data and the conclusions drawn from them. Techniques have been materially improved in these respects in recent years, and I believe we may look forward with confidence to many valuable studies in this field in the near future.

The importance of psychological attitudes for the intelligent study of fertility and population trends is probably most clearly indicated in an analysis of the reported attitudes of individuals in various groups toward family size. In answer to a question asking for their opinion as to the ideal number of children for an American family, the professional groups from which the majority of the data here presented were obtained responded most frequently, "Three." The average of the responses in this group was 3.2. It is very interesting to note that this value is identical with the mean of the responses obtained from people throughout the country in all occupational groups, as found in a survey made by the American Institute of Public Opinion.

It also agrees very well with the value obtained in the survey of women recently made by the *Ladies Home Journal.* The average of these responses was 3.3. This survey, it should be noted, was confined to women, and there is other evidence that the ideal number of children as reported by women is very slightly higher than as reported by men. Probably the most striking fact developed by the analysis of all these data was the very definite negative trend of these responses with the year of birth of the individuals reporting. In both our survey and that of the *Journal,* the average response given by individuals under 30 years of age was about 3.0. This trend is even more strongly indicated in the study of the American Youth Commission in Maryland, reported in the American Council on Education's report "Youth Tell Their Story." The

average number of children which these young people between the ages of 16 and 24 reported they would like to have, if circumstances were ideal, was 2.4.

The implications of such a trend for predictions of population growth are obvious, and a detailed study of the development of this attitude, its basis, and its susceptibility to change is essential. Our studies have shed some light on this subject, though much more work is necessary for even a clear grasp of the salient factors in the situation. The concept of ideal family size seems to be derived from three types of factors: first, the conditions and training of the individual's immediate family; second, the corresponding aspects of the wider social group of which the individual is a part; and third, those characteristics of the individual's personality which are dependent on physical inheritance rather than environmental acquisition. The relation of these various factors to the concept of ideal size of family is shown by our findings. For example, the Pearson product-moment correlation coefficient between reported ideal size of family and the size of the family from which the individuals in our group come, that is, the number of brothers and sisters they had, was 0.33 for the husbands and 0.23 for the wives. Although neither of these coefficients is large, they are both of sufficient size to preclude the possibility of their being errors of sampling. All four correlations of ideal size of family with number of parents' siblings are also positive though somewhat smaller in size.

As a further basis for analysis, the individuals were asked to indicate the importance to them of a number of suggested reasons for having children. The single item which was considered of "great" importance by a majority of both the husbands and wives in the group was "A family is not complete without children." Although similar items reflecting social customs and tradition were given a generally high rank, the *group* of items regarded by both husbands and wives as representing the most important

reasons for having children was that typified by the statement that they enjoyed children and liked having them around. The least important reasons were reported to be those concerned with perpetuating the family name or providing advantages to the parents.

A very interesting study of the relation of various fundamental attitudes toward life to the concept of ideal size of family is afforded by correlating the scores on the Allport-Vernon *Study of Values* blank with the size of family considered ideal. In general, those individuals whose expressed preferences classified them as of a social or religious type favored large families under ideal conditions, and persons who had strong ambitions to attain positions of leadership, or whose interests were strongly economic or esthetic, favored small families.

The discussion thus far has centered around the concept of the desirable number of children under ideal circumstances. To what extent do these variations in ideal concepts actually result in differences in size of family under present conditions? In our professional group the actual size is substantially smaller than the ideal size. In other groups, such as unemployed persons on relief, the actual size is substantially larger than the ideal size. To what extent is size of family actually planned? In our group about 45 per cent report making definite plans concerning number of children shortly after marriage. The proportion making definite plans is substantially larger in the younger groups. It is also evident that the younger group have much more control of the size of their families than the older group. The men in the group who were over 40 years of age report that only 20 per cent of their second-born children were definitely planned, the remainder being either "unplanned" or "accidental." On the other hand, those under 40 years of age report that 49 per cent of their second-born children were definitely planned. About 92 per cent of the group, excluding those reported as sterile, indicate that they have made use of

some method of birth control. That these methods are not entirely effective for these individuals is shown by the fact that even in the younger group 24 per cent of the pregnancies have been accidental, that is, they have occurred despite some attempted method of control.

The opinion voiced by some, that the declining birth rate in this country is evidence of a general loss of virility due to the decadence of our national stock, gains little support from the reports of this group. In response to the question, "If the pregnancy was definitely planned, how many months after you stopped taking precautions did conception occur?" the typical reply is, "Two to three months." It is evident that, although even professional groups do not have complete control over the number of children which they shall have, these groups are approaching such a situation fairly rapidly.

It has already been noted that the number of children which they plan to have is definitely smaller than the number which they say they would favor under ideal circumstances. What then are the reasons for these limitations of family size? The usual reply is that the causes are chiefly financial, and our group is no exception. In addition to this they mention various other reasons such as the health of the wife and the dangers of childbirth, housing conditions, and marital incompatibility.

To discover which facts were actually influential in determining the sizes of families, the reports on estimated size of completed families of two groups, containing 351 married professional men and 289 of their wives, respectively, were compared with other data obtained from them on an anonymous report form. Members of sterile couples were not included.

Various methods have been used in studying the relationships obtained. Possibly one of the simplest and most meaningful analyses is provided by comparing the characteristics of the "small family group" whose completed families are estimated as one or no children with the char-

acteristics of the "large family group" whose completed families are estimated to have three or more children. Thus we have the men in the "small family group" reporting an average "ideal family size" of 2.00 children as compared with 3.12 children for the men in the "large family group." Among the wives, the corresponding figures are 1.87 and 2.89 for the average "ideal family size" of the "small family" women and the "large family" women, respectively.

Another factor related to the expected size of family is the number of brothers and sisters of the individuals in the group. The men in the "small family group" had on the average 3.21 siblings. Those in the "large family group" had 3.83 siblings. Wives in the "small family group" had 2.64 brothers and sisters, while those in the "large family group" had 3.29 siblings.

Religious and other values, as measured by the Allport-Vernon *Study of Values* blank, seem to be related to this planned size of family. Husbands whose scores on religious values fell within the upper 25 per cent of this group plan 2.58 children on the average, while those in the lower 25 per cent plan only an average of 1.79 children. For wives, the average numbers of children planned in these groups are 2.50 and 1.83 respectively. The comparisons of the individuals in the upper and lower quarters on the Political and Esthetic Scales yield smaller differences but they are consistent for husbands and wives. The "high" group of men on the Political Scale plan 2.01 children on the average, while the "low" group plan an average of 2.34 children. For the wives, these figures are 1.98 and 2.51 respectively. Those husbands having scores in the higher quarter of the Esthetic Scale plan an average of 1.94 children, while those in the lower quarter plan an average of 2.34. For the wives, the corresponding averages are 2.03 to 2.32 respectively.

Women who have worried not at all or very little about losing their figures after the birth of the first child expect

to have an average of 2.46 children, but those who had greater worry about this expect to have only 2.00 children on the average. Men whose activities were little restricted by the birth of the first child expect to have an average of 2.49 children, while those who report greater inconvenience anticipate only 2.01 children on the average.

An interesting factor which showed a consistent, though very small, relation with estimated family size for both husbands and wives was their scores on an Optimism Scale. The most optimistic quarter of the husbands and wives expected averages of 2.33 and 2.40 children respectively; the most pessimistic quarter of the husbands and wives expected averages of 2.03 and 2.18 children. The 25 per cent of the groups having the most adequate social adjustment as measured by a personality inventory anticipate averages of 2.00 and 1.91 children for the husbands' and wives' groups respectively. The less adequately adjusted quarters expect averages of 2.21 and 2.31 respectively for the two groups.

Significant differences in planned family size were indicated for those who gave the most and the least weight to certain suggested reasons for having children. The husbands whose scores were in the upper quarter with respect to their ratings of the importance of having children because they enjoy them expect 2.58 children on the average. Those in the lower group expect an average of only 1.71 children. Those who gave more weight to reasons emphasizing the importance of social custom and social pressure expect 2.51 children. Those who gave least value to these reasons expect an average of 1.79 children. Those who consider self-expression as an important reason for having children expect to have an average of 2.48 children, while those who consider this type of reason unimportant expect only an average of 1.88 children. Less significant were the differences in estimated family size for husbands who thought perpetuation of the family important as com-

pared with those who did not, the averages being 2.19 to 2.05 respectively.

For the wives similar relationships were indicated. For the upper and lower quarters with respect to the ratings of importance assigned to the various types of reasons, the average numbers of children expected were respectively: for reasons of social custom and social pressure, 2.36 and 1.81; for reasons concerning the enjoyment of children, 2.19 and 1.87; for reasons related to self-expression, 2.27 and 2.02; and for reasons connected with the perpetuation of the family, 2.19 and 2.03.

Although many of the differences reported are quite small, certain facts should be kept in mind in interpreting them: first, since the size of the family is determined by husband and wife together, the relation to factors involving only one of them must necessarily be quite low; second, the group is a very homogeneous one, with 40 per cent of the completed families estimated at two children and only 10 per cent planning more than three; third, the degree of relation can be expected to increase in size as the effectiveness of birth control methods increases and much of the chance element is taken out of family size.

The findings listed above are for a group which is also quite homogeneous with respect to occupation and income. To investigate the effect of these two factors in a group in which family and social background were controlled, an analysis was made of the number of children born during the first eight years of marriage to the siblings of the individuals in our sample. The average number of children born during the first eight years of marriage to our group was 1.27. The men's siblings had 1.19 children and the women's siblings, 1.54. The average number of children born during the first eight years of marriage to the combined groups of men's and women's siblings was 1.29, which is very close to the average number in our group. When classified by occupational status of the husband, the average number of children born during the first eight

years of marriage to the men's siblings showed little variation, the lowest value being 1.09 for 81 of them in the clerical field, and the highest 1.32 for 99 of them in the managerial field. In general there was no trend of number of children born during this period with occupational status. Similarly, no trend was revealed for this group when the same type of comparison was made with respect to income. As mentioned above, more children were reported for the siblings of the women than for the siblings of the men. Although there was no definite relationship between number of children born during the first eight years of marriage to the women's siblings and the occupational status of the husband, the small number of farmers and the few individuals in the group below the level of skilled workers had more children than did those in the other groups.

An investigation of the usefulness of the "number of children born during the first eight years of marriage" as an index of size of completed family was made by calculating the correlation coefficient between this figure and the number of children ever born to those 55 wives in the group who were over 40 years of age. The correlation coefficient was found to be 0.93. A similar coefficient for the 148 female siblings of the men and women was calculated. This was 0.64. A part of this discrepancy is probably due to actual differences in the groups, but it also seems highly probable that it may be attributed partially to the group's lack of accurate knowledge concerning marriage dates and ages of the children of their brothers and sisters.

Summary

This study of factors relating to fertility has attempted to analyze intensively a relatively small group of about three hundred professional men and their wives, unusually homogeneous with respect to income and background. Not only were data of most of the usual types, such as date and place of birth, nationality of parents, size of parents' fam-

ilies, education, income, etc., obtained, but an effort was also made to secure valid data concerning psychological factors such as attitudes, interests, values, personality traits, etc. In all, about 2,500 separate items of information were obtained for each couple included in this investigation. Some preliminary studies of the relation of these items to fertility were reported. Evidence was presented that the well established fact that professional groups have smaller families than others is not due to a difference in the number of children desired by professional as compared to other groups. Their reports indicate that these professional people, like most other groups in the population, would like to have three or more children under ideal circumstances.

The findings suggest that the sizes of families in this group approximate quite closely their planned size, and that there is a marked trend towards a further decrease in the proportion of children who are not definitely planned for. The factors which appear to be most influential in preventing this professional group from attaining the size of family which they regard as ideal are financial. Perhaps the most important factor is the expense of the type of higher education which those in this group regard as essential. Other considerations which appear to be important are the cost of adequate insurance, housing, and child care.

Another type of factor of importance in limiting families in this group is consideration for the health of the wife.

A finding of some importance is the indication of a definite downward trend with respect to ideal size of family in this as in all other groups for which information is available. Such a trend is bound to exert a strong influence on the average fertility of married couples in this country, especially as it is accompanied by a trend towards decreasing the proportion of unplanned children.

Certain psychological findings which appear to warrant more extensive study in this field are the positive relation-

ship between enjoying children and liking to have them around with planned size of family; the larger planned families among the individuals classified as of the Social and Religious types as compared to those planned by individuals of the Political, Economic, and Esthetic types; the larger families expected by those wives who are less adequately socially adjusted, as compared to the families anticipated by the well adjusted wives. Two findings which throw some light on the well established negative relationship between size of family and economic status are, first, the fact that while in this professional group of individuals actual size of family is smaller than ideal size, in groups who are on "relief" the actual size of family is larger than these individuals believe is ideal; and secondly, that among the siblings of this professional group, there was no trend of family size with occupational or income status.

The theory that the decline of the birth rate is fundamentally due to decadence and loss of virility in the stock of this country is controverted by the fact that the interval required for conception among the couples in our group was on the average only two or three months.

In conclusion, it should be stated that the significance of this investigation lies to a greater extent in the use of a new approach to the study of population problems than in the finality of the findings reported for this sample of professional men and their wives.

VOLUNTARY AND INVOLUNTARY ASPECTS OF CHILDLESSNESS

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(Read November 18, 1938, in Symposium on Population Growth)

THE Book of Genesis documents the existence of barren women in early Biblical times. Despite the age-old character of the problem, we know little about the incidence of actual sterility today. There are some data regarding the proportion of childless families among marriages of completed fertility, but such figures leave unanswered the question concerning the extent to which such childlessness is voluntary and the extent to which it represents physical inability to bear a child. In view of the researches of Reynolds and Macomber[2] and others, it must be recognized at the outset that sterility is a relative term. There is not in all cases a clear line of demarcation between voluntary and involuntary childlessness, and there is a small but constant passage of childless women from the former into the latter category. Despite this, few will deny the meaningful nature of the voluntary-involuntary dichotomy of childless families of completed fertility, and the dearth of knowledge of the relative importance of these groups is often lamented by students of medical problems, students of population, and by social scientists in general.

Two investigations bearing directly upon this question appear to have yielded diametrically opposite results. On

[1] From the Milbank Memorial Fund. The author wishes to express his indebtedness to the National Committee on Maternal Health for its active assistance in collecting data for this study. He is particularly grateful to Dr. Raymond Squier of that organization.

[2] Reynolds, E. and Macomber, D.: *Fertility and Sterility in Human Marriages.* Philadelphia, W. B. Saunders Co., 1924, p. 155 ff.

the one hand, an analysis presented by Lorimer and Osborn [1] has given suggestive evidence that from two-thirds to three-fourths of the 60 childless women of completed fertility included in the Davis [2] series may be described as involuntarily childless. In contrast are the findings reported by Popenoe [3] who "asked more than 100 adult students at the . . . University of Southern California to list all the cases of permanent childlessness that they knew among their closest friends and relatives, selecting only such marriages as they felt sure would at no time in the future produce children, and only those couples whom they knew so intimately that they felt no doubt as to the motivation of childlessness." Of 862 histories of couples contributed, two-thirds were listed as voluntarily childless. Women of superior social status predominated in this group as well as in that studied by Lorimer and Osborn. Popenoe's sample was considerably larger but appeared to be unduly weighted by women who were pursuing a career. In regard to this situation, Popenoe stated: "This classification is large because so many of my students are teachers, social workers, and the like, and report the cases of their own friends in the professions." The authors of both studies are modest in the evaluation of their results and emphasize the need for further investigation along this line.

An opportunity to secure an assay into the problem in New York City has been provided through the cooperation of the National Committee on Maternal Health. It should be emphasized, however, that this study was mainly experimental from the onset, and that the final usable sample was small, was confined to New York City, and was not otherwise free from limitations. On the other hand, the

[1] Lorimer, F. and Osborn, F.: *Dynamics of Population.* New York, The Macmillan Company, 1934, pp. 257–258.

[2] Davis, Katherine B.: *Factors in the Sex-Life of Twenty-two Hundred Women.* New York, Harper and Bros., 1929.

[3] Popenoe, Paul: "Motivation of Childless Marriages." The *Journal of Heredity,* xvii, No. 12, December, 1936, pp. 469–472.

data possess certain advantages over those collected in the two investigations previously mentioned.

The present study was prompted by the availability of family records from a health survey in which the Milbank Memorial Fund participated during the fall and winter of 1935–1936. That sample covered 48,000 households in New York City and is believed to be fairly representative, for it purported to include every thirty-sixth domicile listed in the Real Property Inventory's file of occupied houses and apartments in New York City. In that survey a census of persons in the household was the starting point for the present supplementary study of childlessness. It was not possible to identify couples who had never borne a child but it was possible to spot heads of households and their wives who reported no children in residence. Such couples were chosen for further investigation if they were white, if the wife was under fifty years of age, and if the couple had been married ten years or more (as determined by length of time household had been established). These restrictions served to eliminate elderly couples whose children had left home, and young childless couples who were likely to have children in the future; and they automatically ruled out the possibility of including women who were forty years of age or older at marriage. The names, addresses and descriptive data concerning socio-economic status were copied for 2,250 couples meeting the above requirements and residing in the four main boroughs of the city. These cases are designated in Table 1 as the "original transcripts."

Two methods of study were utilized—the mailed questionnaire and the interview. The plan adopted was to solicit required data from the total group by a mailed questionnaire and later to conduct personal visits among a random group of women who had not replied by mail.

The questionnaire adopted provided for entries concerning age, years married, whether or not the wife had ever borne a child and, if not, whether she had ever been

pregnant. The women who had never experienced a pregnancy were asked to check one of three stated possibilities with respect to the extent of contraceptive practice since marriage. Provisions were also made for indicating whether the failure to have a child had been a disappointment and whether the childless women had ever gone to a doctor to learn why they could not have a child. The recipients were not required to sign their names but a system of identification was devised whereby it was possible to match the returned questionnaires with the descriptive data on the original transcripts and hence to test the socio-economic representativeness of the women who replied by mail.

TABLE 1

DISTRIBUTION BY BOROUGH OF ORIGINAL TRANSCRIPTS, CLIENTS RECEIVING QUESTIONNAIRE FORM AND CLIENTS RETURNING FORM BY MAIL

	Four Boroughs	Brooklyn	Bronx	Manhattan	Queens
Original Transcripts	2,250	668	449	657	476
No Contact	273	78	54	109	32
Clients Receiving Form	1,977	590	395	548	444
Clients Returning Form	159	43	37	49	30
Per Cent Replies	8.0	7.3	9.4	8.9	6.8

As shown in Table 1, of the 2,250 women represented in the original transcripts, 1,977 presumably received the questionnaires by mail. The remaining 273 [1] were not reached because they had moved without leaving a forwarding address. Of the 1,977 actually contacted, only 159 (or 8 per cent) filled in and returned the questionnaire.

[1] These include the instances in which no delivery was attempted by virtue of previous clearance of addresses for Brooklyn, The Bronx and Manhattan addresses through the two central post offices serving those boroughs, and also include the unsuccessful attempted deliveries represented by the returned, unopened envelopes marked "not living at this address." The questionnaires were sent out during the summer of 1937, between one and two years after the original survey.

Furthermore, since the group of women who sent replies by mail was found to be unduly weighted by individuals of superior socio-economic status, the necessity of another approach was clearly indicated.

As shown in Table 2, a sample of 617 women was drawn at random for the follow-up investigation by personal interview. Two carefully chosen trained nurses [1] were employed to do this field work during the fall and early winter of 1937–1938.

Upon completion of the nurses' visits we had from the 617 women in the random sample a total of 459 usable schedules, 405 of which were supplied by nurses and 54 of

TABLE 2

NUMBER AND PER CENT OF WOMEN DRAWN AT RANDOM FOR FOLLOW-UP STUDY, BY BOROUGH

	Four Boroughs	Brooklyn	Bronx	Manhattan	Queens
Clients Receiving Form....	1,977	590	395	548	444
Number in Random Sample..	617	173	117	194	133
Per Cent in Random Sample.	31.2	29.3	29.6	35.4	30.0

which were mailed replies. As indicated in Table 3, the total figure represents 74 per cent of the random sample for the four boroughs combined. No schedules were secured for the remaining 26 per cent for reasons indicated in the table. Table 3 also shows that the field work was not uniformly successful in the four boroughs, the percentage of returns extending from 60 per cent in Queens [2] to 86 per cent in Brooklyn.

[1] The writer wishes to express his indebtedness to Miss Jean Aldrich, R.N., and to Miss Maude Lyle, R.N., for their competent work connected with this study.

[2] It will be noted that in Queens a relatively high proportion of women were designated as "not found." This proportion would doubtless have been lowered somewhat had it been possible to devote as much time to revisits as was pôssible in the remaining boroughs where five or six recalls were often made (including some evening and Sunday work). The nurses' visits, however, were terminated when both resigned to take permanent positions elsewhere. Preliminary analysis of the type presented in Figs. 1–4 indicated

TABLE 3

NUMBER AND PER CENT OF RETURNS FROM WOMEN IN THE RANDOM SAMPLE FOLLOW-UP AND REASONS FOR FAILURES TO SECURE RETURNS

Analysis of Returns and Non-Returns	Four Boroughs	Brooklyn	Bronx	Manhattan	Queens
	Number of Women				
Total in Random Sample	617	173	117	194	133
Returns Secured	*459*	*149*	*100*	*130*	*80*
By Mail	54	16	14	14	10
From Nurses' Visits	405	133	86	116	70
Returns Not Secured	*158*	*24*	*17*	*64*	*53*
Client Had Moved	71	12	11	26	22
Not Found at Home [1]	40	1	—	18	21
Refused	44	11	6	18	9
Deceased	3	—	—	2	1
	Per Cent				
Total in Random Sample	100.0	100.0	100.0	100.0	100.1
Returns Secured	74.4	86.1	85.5	67.0	60.2
Returns Not Secured:					
Client Had Moved	11.5	6.9	9.4	13.4	16.5
Not Found at Home	6.5	0.6	—	9.3	15.8
Refused	7.1	6.4	5.1	9.3	6.8
Deceased	0.5	—	—	1.0	0.8

[1] Not found at home but living at address.

Since the nurses did not succeed in procuring all required returns, questions arise concerning the type of bias inherent in this situation. In so far as socio-economic status is concerned, the women who granted interviews

that the combination of the nurses' records with the replies sent by mail yielded a group closely similar to the original universe in so far as socio-economic status is concerned; so it was not deemed necessary to hire a new person for purposes of reducing the number of women designated as "not found" in Queens.

were more representative than were those who replied by mail. The bias that did exist, however, was in a direction opposite to that observed among women who replied by mail. In other words, the nurses failed to secure the ex-

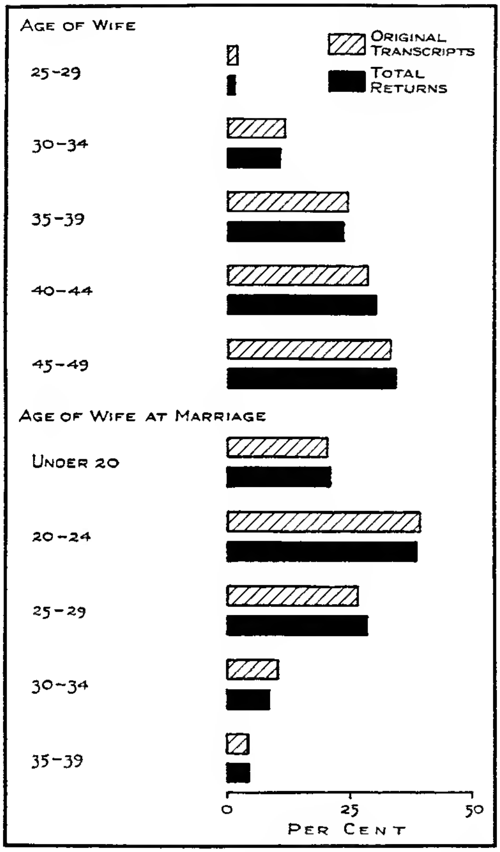

FIG. 1. Percentage distribution by age and age at marriage: wives submitting returns for study of childlessness compared with all included in the original transcripts.

pected quota of schedules from women in the upper classes and consequently their returns were weighted somewhat from women in the lower strata.

By virtue of the above type of counter-bias, the combination of all returns secured by mail and through nurses'

visits, a total of 564, affords a sample closely resembling the original universe in so far as descriptive and socio-economic attributes are concerned. The similarity in the composition of the two groups, with respect to age of wife and age of wife at marriage, is apparent from virtually identical lengths of the shaded and solid bars in Fig. 1.

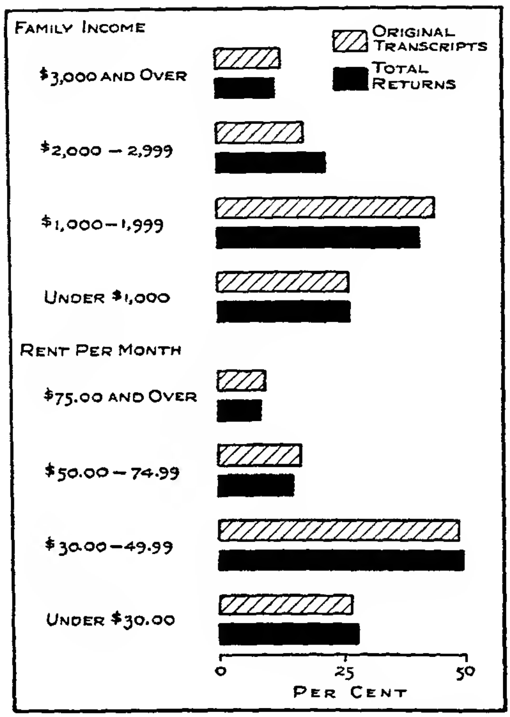

FIG. 2. Percentage distribution by total family income and by monthly rental: wives submitting returns for study of childlessness compared with all included in the original transcripts.

Likewise, Fig. 2 represents the comparative distributions with respect to family income and monthly rental; Fig. 3 with respect to occupation of the husband, and Fig. 4 with respect to educational attainment of the husband and wife.

It should be emphasized that the above tests for représentativeness refer only to certain descriptive attributes and afford no guarantee of representativeness in so far as

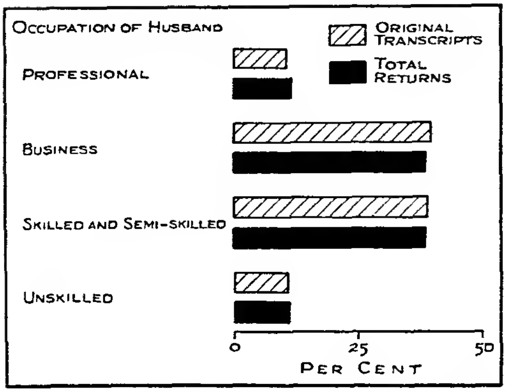

Fig. 3. Percentage distribution by occupational class of the husband: wives submitting returns for study of childlessness compared with all included in the original transcripts.

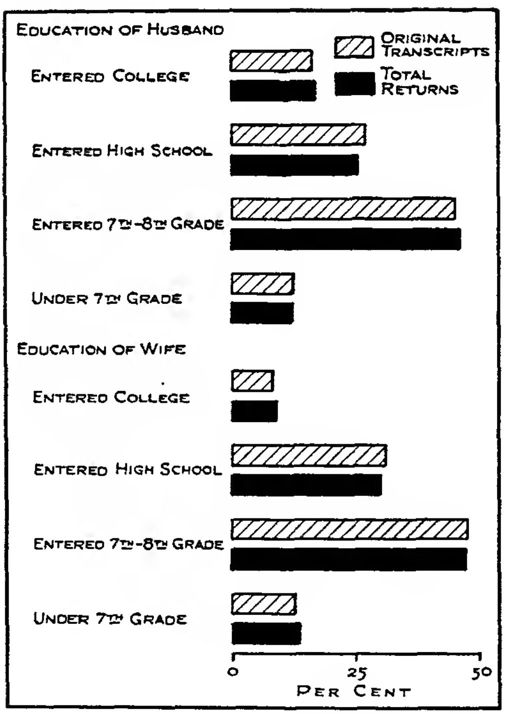

Fig. 4. Percentage distribution by education of the husband and of the wife: wives submitting returns for study of childlessness compared with all included in the original transcripts.

voluntary and involuntary aspects of childlessness are concerned. In this connection we can only say that the postulation of representativeness in the latter respect would be based upon poor ground indeed had the sample differed substantially from the original universe in so far as socioeconomic status is concerned.

Among the 564 women for whom records were secured, there were 411 who reported that they had never borne a child. These childless women, in turn, were distributed as follows: 291 reported that they had never been pregnant; 90 [1] reported a previous pregnancy although they had never borne a child, and 30 [2] gave no report concerning previous pregnancy. Percentage distributions, based upon reported histories of previous pregnancies, indicate that a little more than three-fourths of the childless women considered here had never conceived, the range by borough extending from 71 per cent in Queens to 82 per cent in Brooklyn.

Attention is now directed to the replies submitted by the never-pregnant women, for these women alone were requested to answer the questions concerning contraception. The data are consistent in their implications that absence of conception among couples married ten years or more is in large part an involuntary situation.[3] In this respect the findings are in agreement with those of Lorimer and Os-

[1] The questionnaires did not require specification of type of pregnancy wastage. This information, however, was given more or less specifically by a substantial number of women, especially among those interviewed by nurses. The 90 women were distributed as follows: 17 had one or more stillbirths but no live births; 5 reported tubal pregnancies; 32 specified one or more spontaneous abortions; 7 specified induced abortions; 9 reported "abortions" but did not specify type, and 20 gave no information aside from the report that they had never borne a child but had been pregnant.

[2] In addition to failure or refusal to divulge information additional to the fact of childlessness, there are included in the above a few instances in which the nurses failed to contact the women but learned from other sources (mothers, sisters) that the women in question had never borne a child. Information from such secondary sources was not solicited, or accepted for data other than those concerning previous childbirth.

[3] In considering the implications of a high proportion of infecundity among the childless couples in this sample, it should be emphasized that we are here concerned with the experience of the *mated couple*, not with potential fecundity of either the wife or husband.

born, not with those of Popenoe. As indicated in Fig. 5, over three-fourths of the never-pregnant women reported that neither they nor their husbands had ever done anything since marriage to prevent conception; about 9 per cent stated that only temporary or occasional practice of contraception had been employed, and approximately 14 per cent reported regular and continuous practice. Fur-

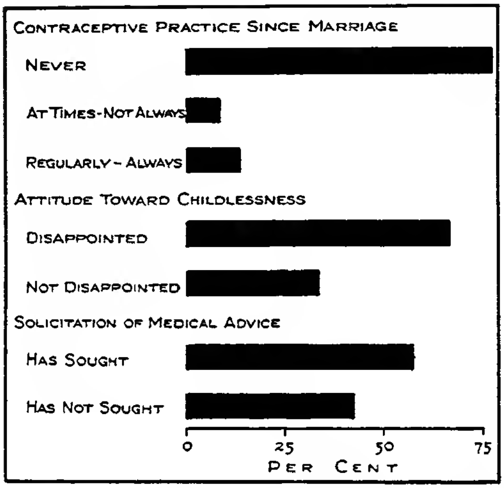

FIG. 5. Distribution of never-pregnant women submitting returns for study of childlessness, according to practice of contraception since marriage, expression of disappointment over childless condition, and by previous solicitation of medical advice.

thermore, two-thirds of the never-pregnant women reported that they were disappointed in their childless condition and 57 per cent declared that they had, in the past, consulted a physician to ascertain why they could not have a child.[1]

At first it may appear that the proportion of never-

[1] The major importance of involuntary factors is likewise implied when all childless women in the sample (not simply the never-pregnant women) are considered. As previously stated, the questionnaires requested only from never-pregnant women data concerning practice of contraception since marriage. The childless women who had been pregnant, however, submitted these data and those concerning attitudes and solicitation of medical advice in sufficiently large proportions to warrant suggestive presentation. The percentage distributions (at the right of the table below) for the total 411 childless

pregnant women reporting no contraceptive practice is incredibly high. Pearl[1] found that only 47 per cent of 3,420 white maternity cases delivered in hospitals in New York City reported *no* previous contraceptive practice. Nevertheless, the present findings are not inconsistent with those of Pearl. All of the women in Pearl's sample had been

women and for the 90 who reported previous pregnancies are shown as computed, (*a*) with the "unknowns" included in the base, and (*b*) with the "unknowns" excluded. In this sense it can be said that among the total group of childless women, from two-thirds to three-fourths never practiced contraception. Even if all the unknown cases are assumed to be regular users of contraception, only 26 per cent of the total childless would fall into this category. Likewise, from 60 to 70 per cent of all childless women were "disappointed" with their condition, and from 50 to 60 per cent had consulted a physician to find out why they could not have a child.

Contraceptive Practice since Marriage, Attitude toward Childlessness, and Solicitation of Medical Advice	Number				Per Cent			
	Total Childless	Previous Pregnancies			Total Childless		Has Been Pregnant	
		None	One or More	Unknown	Unknowns Included	Unknowns Excluded	Unknowns Included	Unknowns Excluded
Total	411	291	90	30	100.1	100.0	100.0	100.0
Contraception								
Never Practiced	269	217	51	1	65.5	76.2	56.7	69.9
At Times	36	24	12		8.8	10.2	13.3	16.4
Regularly and Always	48	38	10		11.7	13.6	11.1	13.7
Unknown	58	12	17	29	14.1		18.9	
Disappointment								
Total	411	291	90	30	100.1	100.0	100.0	100.0
Disappointed	247	187	59	1	60.1	68.8	65.6	76.6
Not Disappointed	112	93	18	1	27.3	31.2	20.0	23.4
Unknown	52	11	13	28	12.7		14.4	
Medical Advice								
Total	411	291	90	30	100.0	100.0	100.0	100.0
Has Sought	204	157	43	4	49.6	60.0	47.8	72.9
Has Not Sought	136	117	16	3	33.1	40.0	17.8	27.1
Unknown	71	17	31	23	17.3		34.4	

[1] Pearl, Raymond: "Fertility and Contraception in New York and Chicago." The *Journal of the American Medical Association,* cviii, April 24, 1937, pp. 1385–1390.

pregnant at least once, whereas our present consideration is restricted to women who were never pregnant after ten years or more of married life. This, together with the fact that Pearl's cited analysis is confined to women free from any form of gynecologic disease, constitutes a cardinal difference between the two sets of data. It is well known that contraceptive practice increases with order of pregnancy. Thus, although only 40 per cent of the multiparous women in Pearl's sample reported *no* contraceptive practice since marriage, 61 per cent of the primiparous women in his sample gave such report. It therefore may not appear unreasonable that 78 per cent of the never-pregnant women in the present sample should report themselves as never having practiced contraception. This is especially true when it is considered that the present sample does not exclude couples who have never practiced contraception due to their knowledge that such efforts were unnecessary.

The volunteered comments on the schedules confirm the indication of involuntary childlessness among women who have never conceived. Among the 291 who were never pregnant there were 158 who stated in specific or in general terms the nature of a pertinent operation or affliction, *or* stated that they had been advised *by a physician* that there was "no apparent reason" for failure to conceive. It is not without interest that the above number of women who volunteered such information constituted 54 per cent of the total number of women who had never conceived. If the number includes lay opinions and, therefore, imagined ailments, it also excludes an unknown number declining to comment on real physical disorders accounting for sterility.[1]

[1] The following grouping of the 158 women according to reported pathology is presented merely as a suggestion of their collective, not individual, importance: history of tubal infection, 9; uterine operations performed or advised, 26; ovarian pathology, 8; severe abdominal and/or pelvic infections, 5; infantile uterus, 12; uterine displacement, 15; endocrine condition, 5; hus-

Lack of time prevents more than brief mention of the variations in extent of contraceptive practice within the group of never-pregnant women. The small size of the sample precluded dependable results, but in general it appeared that the proportion reporting no contraceptive practice was a little lower among the native whites than among those of foreign birth, lower among women in the younger age groups than among those in the older, and lower among women of most educated groups than among their opposites. The group differences in proportions expressing disappointment and in proportions claiming past solicitation of medical advice were smaller and of less consistent nature. Of central importance, however, was the fact that in all strata the absence of conception after ten years or more of married life appeared to be largely an involuntary situation. Moreover, a surprisingly high number of women had sought medical advice to ascertain why they had failed to conceive.

Questions may arise concerning the possible importance of late age at marriage among the never-pregnant women reporting no contraceptive practice. By virtue of the original restrictions, of course, all of these women were under 40 at the time of marriage. The actual distribution shows that 13 per cent were under 20, 57 per cent were under 25, and 87 per cent were under 30. For purposes of a control, these figures were compared with similar data for a group of Brooklyn white women from an altogether different survey and unselected with respect to fertility or contraceptive practice. In general, such a comparison indicates that late age at marriage was a factor but was not of primary importance in the childless condition of this group of never-pregnant women.[1]

band sterile, 10; operations designed to facilitate pregnancy, advised or performed, 24; "being treated," "natural causes," "general debility," "not properly mated," etc., 23; *physician advised* there was "no apparent reason" for failure to have a child, 21.

[1] Below are presented percentage distributions according to age at marriage for total never-pregnant women, for never-pregnant women reporting no

For a really definitive study we need for a random group of childless couples detailed social and medical records, including complete histories of contraceptive practice and any pregnancy wastage, studies of attitudes and, most of all, medical data similar to those now existing only for the selected group of childless couples who consult specialists. Until such data become available, investigations somewhat similar to the present are needed in other areas. Pending further studies the writer's tentative conclusion is that however prevalent may be the practice of contraception for purposes of postponing and spacing pregnancies, such practices cannot be held responsible for any major share of permanent childlessness. One recipient of the questionnaire unwittingly touched upon the central indications of this study when she wrote the following comment: "I firmly believe that most childless women are physically unable to have children and to (*sic*) poor to go through treatments. For life without children is a very dreary dissatisfyed (*sic*) life, judging by myself and friends."

practice of contraception, and for a control group unselected with reference to previous pregnancies and contraceptive practice:

Age at Marriage	Total Never Pregnant	Never Pregnant No Contraception	Brooklyn Survey (Control)
Total	99.9	99.9	100.0
Under 20	12.9	12.9	31.8
20–24	39.9	44.0	45.8
25–29	33.1	30.6	16.8
30–34	10.4	9.1	4.9
35–39	3.6	3.3	0.7

MORTALITY IN RELATION TO WIDOWHOOD

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(*Read November 18, 1938, in Symposium on Population Growth*)

SINCE the family is the social unit through which the growth of population is influenced, a certain interest attaches to a measurement of chances that the family will be broken by the death of the husband or wife. This interest may properly be extended beyond the child-bearing period of the wife into the period during which the child still needs the benefits of care from the parents.

There are three approaches to the problem of measuring the chances that the family may be broken:

First, for any combination of ages of husband and wife, it is possible, by a suitable selection of life tables, to measure the probability that the couple either will or will not survive jointly for any number of years. Computations of this kind are usually based on life tables for total persons since it is not possible to construct survivorship columns from the mortality rates for married persons.[1] But any computation carried out by the aid of life tables for total persons is subject to error because the mortality for married persons is usually lighter than that for total persons.

Second, the chances that a married person will die within the course of the year and thus *leave* a broken family are

[1] The derivations of formulæ involving the probabilities of life and death for two or more lives are given in E. F. Spurgeon, *Life Contingencies*, Cambridge Press, chapters XIII and XIV. Some results of a computation involving two lives are given in "The Chances of Celebrating a Golden Wedding," *Statistical Bulletin of the Metropolitan Life Insurance Company*, **15**, No. 11 (November 1934), p. 1; see also M. Spiegelman, "The Broken Family—Widowhood and Orphanhood," *Annals of the American Academy of Political and Social Science*, **188** (November 1936), p. 117.

measured by the entries in a table of age-specific rates of mortality for *married* males and females. In this case, we do not inquire into the age of the surviving spouse. Two tables of this kind are available in the United States; namely, one relating to the United States Death Registration States of 1924 for the period 1924 to 1928,[2] and the other based upon the experience of New York State, exclusive of New York City, in the period 1929 to 1931.[3]

According to a third approach, which forms the subject of this paper, the chances that a husband or wife *will be left* with a broken family by the death of the spouse within the course of the year may be measured by means of a table of age-specific annual rates of widowhood.[4] The preparation of a table of this kind would ordinarily require data regarding the age of the surviving mate at the death of the husband or wife, and, in addition, the number of wives or husbands in the population, also distributed by age. Since data of the first kind are not collected, use will be made of an indirect method for the computation of age-specific rates of widowhood.

Rates of Widowhood by Indirect Method

The nature of the indirect method for the computation of age-specific rates of widowhood was clearly described by Wolfenden in 1917.[5] ". . . the rates could be found, by making use of the mortality rates of married men, if the census reports classified by ages the husbands of the married women (or vice versa) of each age, or an approximation could be obtained if the average age of the husbands of the married women of each age were known. . . ." In other words,

[2] Walter T. Willcox, "Introduction to the Vital Statistics of the United States, 1900 to 1930," Bureau of the Census, Washington, D. C., 1933, p. 43.

[3] "Marriage and Long Life," *Statistical Bulletin of the Metropolitan Life Insurance Company*, 18, No. 2 (February 1937), p. 7.

[4] By the term age-specific "rate of widowhood" is here meant the annual rate at which women of a given age become widowed by the death of their husbands of unspecified age.

[5] H. H. Wolfenden, "Observations on the Methods and Publications of the United States Census Bureau," *Transactions of the Actuarial Society of America*, 18, Part 2, No. 58 (1917), pp. 260–286.

according to the first of these two alternatives, the rate of widowhood for married women at any age is equivalent to the average rate of mortality among their husbands, the weights used in computing this average being the frequency distribution of their husbands according to age. This alternative will be described at length; an example will also be given of the second alternative. With appropriate changes, the method may be applied to find rates of widowhood for married males.

From a general consideration of the mortality element in the method of the preceding paragraph, it appears that the computation will yield results which overstate the true rates of widowhood at the younger ages and understate them at the older ages. This will be readily understood when it is realized that the mortality rates used in the computation do not make any allowance for the following:

(1) A table of mortality rates for married persons which would take into account, not only attained age, but also duration since marriage, may be expected to show that, at each age, the mortality rates would increase with lapse of time since marriage. This would follow from the possibility that the fair state of health presumed to exist at the time of marriage may deteriorate as time goes on. Thus, the rates of mortality for married persons in a table which distinguishes by age only will be too high as far as recent marriages are concerned, and too low as far as lengthy marriages are concerned.[6]

(2) Among wives at the younger ages, irrespective of the age of the husband, the average duration of marriage is less than among wives of older ages, and, mutatis mutandis, for husbands.

In view of these two circumstances, it will be seen that, in the case of young wives, the assumption that their husbands will experience the mortality rates of married men generally

[6] It is partly for this reason that the customary survivorship column of a life table cannot be computed from a column of age-specific rates of mortality for all married persons, irrespective of duration since marriage.

will result in rates of widowhood that are too high, and, in the case of older wives, the same assumption will yield rates of widowhood that are too low. Again, a corresponding situation will be found in the computed rates of widowhood for husbands. At present, there seems to be no means of overcoming, or even estimating, this limitation of the method, nor is it believed that it will seriously distort the true picture. However, the limitation should be borne in mind in construing the results presented here.

Basic Data: Age Distribution of Husbands and Wives

Through the courtesy of the Bureau of the Census, an unpublished table showing the age distribution of husbands according to age of wife was made available. The data, obtained from the schedules of the 1930 census, related to native white families in Pennsylvania in which husband and wife were both present. Figures were given for each age of the husband between 20 and 65 years; nothing was available for ages outside of this range. The age classification for the wives was: under 20, 20 to 24, 25 to 29, single ages from 30 to 64, and a final group of ages 65 and over (including those of unknown age). To facilitate the computations, all data for single years of age were grouped into five year periods.

Since the method requires data for the entire range of ages of husbands and wives, it was necessary to approximate data for husbands over age 65 in five year age groups, with a further classification, for each such age group, according to the ages of their wives. It was also necessary to approximate data for wives over age 65 in five year age groups, in this case showing, for each such age group, a further distinction according to the ages of their husbands. For greater clarity, reference should be made to Table I; the figures enclosed in the heavy black frame are census tabulations, while those outside the frame have been estimated. For purposes of estimation, it was assumed that the table of age distribution of husbands and wives corresponded essentially to the situation in a stationary population. Preliminary estimates for

five year age groups over age 65 were obtained, on a joint survival basis, from the figures given in the column and the row of the table corresponding to ages 60 to 64. In the case of husbands of ages 60 to 64, the joint survival factors vary with the age of the wife; in the case of wives of ages 60 to 64, the joint survival factors vary with the age of the husband.[7]

For husbands of ages 60 to 64, the joint survival factors take the form of

$$\frac{\sum_{t=0}^{t=4} L_{60+k+t}^{male}}{\sum_{t=0}^{t=4} L_{60+t}^{male}} \cdot \frac{\sum_{t=0}^{t=4} L_{y+k+t}^{female}}{\sum_{t=0}^{t=4} L_{y+t}^{female}}, \tag{1}$$

where the function L represents the number of years of life lived by the life table cohort (male or female, as the case may be) within the year of age specified by the subscript; y represents the age of the wife; and k represents the number of years for which the joint survival probability is desired. For the present purposes, k was given values of 5, 10, 15, etc.

In the case of wives of ages 60 to 64, the survival factors take the form of

$$\frac{\sum_{t=0}^{t=4} L_{60+k+t}^{female}}{\sum_{t=0}^{t=4} L_{60+t}^{female}} \cdot \frac{\sum_{t=0}^{t=4} L_{x+k+t}^{male}}{\sum_{t=0}^{t=4} L_{x+t}^{male}}, \tag{2}$$

where x represents the age of the husband. By multiplying the factors derived from expression (2) by the figures in the column for wives of ages 60 to 64 and by multiplying the factors derived from expression (1) by the figures in the row for husbands of ages 60 to 64, estimates were obtained for the columns and rows corresponding to the higher quinquennial

[7] The joint survival factors were computed from life tables for white males and white females in Pennsylvania, 1929 to 1931. These are published in "Population Statistics, No 2, State Data," by the National Resources Committee, October 1937.

age groups. The resulting estimates for husbands of ages 60 and over were then adjusted downward so that the total of husbands with wives present would be 93 per cent of the number of native white married males reported for Pennsylvania in the census of 1930.[8] The age distribution of husbands and wives used in the computation of the rates of widowhood is shown in Table I.[9]

Basic Data: Mortality Rates

In order to learn what effect improvement in mortality has upon the rates of widowhood computed by the indirect method, the mortality rates for persons of all marital conditions from the following tables (*a*) to (*c*) were used:

(*a*) English Life Table No. 3, 1838–1854

(*b*) United States Original Registration States, white persons, 1901

(*c*) United States, excluding South Dakota and Texas, white persons, 1929–1931

A computation was also performed on the basis of:

(*d*) Pennsylvania mortality rates for native white married males and females, 1929–1931. Since these mortality rates are not obtainable from observed population and deaths, they were approximated from the mortality rates for total white males and total white females in Pennsylvania, 1929–1931, by assuming

(1) that the differential [10] in mortality between the native born and the foreign born at each age in Pennsylvania is the same as that in the Original Death Registration States in 1929

[8] Apparently not all native white married males were living with their wives. There is also the possibility that some reported as single were actually widowed or divorced. The number of native white males of ages 60 to 64 with wives present, as shown in the unpublished table, was found to be 93 per cent of the native white married males of the same ages as reported for Pennsylvania in the census of 1930.

[9] The figures for husbands under age 20 are arbitrary.

[10] The term "differential" is here used in the sense of a ratio between mortality rates of the population classifications specified.

TABLE I

DISTRIBUTION OF NATIVE WHITE FAMILIES IN PENNSYLVANIA WITH HUSBAND AND WIFE BOTH PRESENT, ACCORDING TO AGE COMBINATIONS, 1930

Age of Husband, X	Age of Wife, Y																
	Total	Under 20	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65–69	70–74	75–79	80–84	85–89	90–94
Total.....	1,271,718	21,515	118,324	171,732	185,032	179,127	151,123	130,818	108,910	81,870	56,127	35,111	18,741	7,383	1,925	297	23
Under 20.	2,430	2,200	200	30													
20–24 .	60,651	13,155	11,364	5,557	392	70	30	25	22	22	11						
25–29.	146,414	4,789	56,926	72,730	10,330	1,155	216	83	77	55	41	12					
30–34...	180,170	856	11,840	69,174	80,703	12,421	1,556	301	115	80	75	34	9				
35–39...	185,036	268	3,369	20,320	67,328	79,107	12,130	1,816	395	135	76	61	25	6			
40–44 ..	159,287	116	959	4,615	18,523	59,151	63,680	10,026	1,613	352	117	62	45	16	3		
45–49...	140,567	71	378	1,411	5,122	18,875	49,618	53,883	9,131	1,561	283	93	45	28	8		
50–54 .	122,344	28	163	508	1,711	5,445	16,756	43,710	45,046	7,443	1,171	222	66	28	14	3	
55–59 .	98,709	22	87	242	602	1,938	4,938	14,872	35,739	33,821	5,315	890	154	40	14	5	
60–64.	74,603	10	38	87	236	727	1,764	4,308	12,531	26,862	23,933	3,175	528	79	17	5	
65–69. .	49,667			28	61	174	532	1,280	3,079	8,736	18,022	15,164	2,249	296	36	6	1
70–74 .	31,060				21	48	128	388	918	2,154	5,882	11,159	8,800	1,131	119	11	1
75–79..	11,756					13	29	77	230	532	1,201	3,096	5,505	3,663	379	29	2
80–84...	4,909						7	11	36	105	233	498	1,170	1,803	965	74	4
85–89..	1,005							2	5	11	32	69	134	273	338	134	7
90–94..	110									1	2	6	11	20	32	30	8

NOTE: Figures enclosed in heavy frame are census data; figures outside of heavy frame are extrapolated (see text).

to 1931, for which mortality tables by nativity are available.[11]

(2) that the differential [10] in mortality between the single, married and widowed of the native born in Pennsylvania at each age is the same as that in New York State (exclusive of New York City), 1929 to 1931.[3]

To explain the method of approximation it will be necessary to introduce the following symbols:

Let q denote the mortality rate at any age x;
s, m, and w be superscripts attached to the right of q to show that the mortality rate relates to single, married, or widowed (including divorced) lives;
t, n, and f be subscripts attached to the right of q to show that the mortality rate relates to total white persons, native white persons, or foreign-born white persons;
p denote the proportion of persons of age x who fall in the categories s, m, w, or n and f, so that

$$p^s + p^m + p^w = 1,$$
$$p_n + p_f = 1.$$

Then, for either sex,

$$\begin{aligned} q_n &= p_n^s q_n^s + p_n^m q_n^m + p_n^w q_n^w \\ &= q_n^m(r^s p_n^s + p_n^m + r^w p_n^w), \end{aligned}$$

where r^s and r^w are defined by

$$q_n^s = r^s q_n^m \qquad \text{and} \qquad q_n^w = r^w q_n^m.$$

In the same way

$$\begin{aligned} q_t &= p_n q_n + p_f q_f \\ &= q_n(p_n + r_f p_f), \end{aligned}$$

where r_f is defined by

$$q_f = r_f q_n.$$

Hence,

$$q_n^m = \frac{q_t}{(p_n + r_f p_f)(r^s p_n^s + p_n^m + r^w p_n^w)}.$$

[11] These tables were presented in abridged form by L. I. Dublin at the International Congress of Population, held at Paris, 1937, and published in Vol. 5, page 188 of their proceedings.

TABLE II

ESTIMATED DEATH RATES PER 1,000 NATIVE WHITE MARRIED MALES AND FEMALES IN PENNSYLVANIA,* 1929 TO 1931

Age	Males	Females	Age	Males	Females
15	1.35	1.71	55	17.60	14.27
16	1.48	1.93	56	18.95	15.36
17	1.63	2.18	57	20.38	16.55
18	1.77	2.44	58	21.90	17.82
19	1.92	2.71	59	23.53	19.18
20	2.06	2.96	60	25.30	20.64
21	2.19	3.21	61	27.24	22.22
22	2.30	3.40	62	29.39	23.96
23	2.42	3.49	63	31.74	25.85
24	2.53	3.51	64	34.35	27.92
25	2.64	3.50	65	37.18	30.21
26	2.76	3.48	66	40.23	32.78
27	2.87	3.49	67	43.49	35.65
28	2.98	3.53	68	46.99	38.89
29	3.11	3.62	69	50.78	42.53
30	3.25	3.71	70	54.93	46.56
31	3.42	3.82	71	59.51	50.93
32	3.60	3.94	72	64.59	55.63
33	3.79	4.07	73	70.13	60.53
34	4.02	4.21	74	76.25	65.86
35	4.28	4.37	75	82.94	71.64
36	4.58	4.55	76	90.17	77.93
37	4.92	4.73	77	97.95	84.79
38	5.30	4.93	78	106.27	92.25
39	5.71	5.15	79	115.12	100.24
40	6.15	5.38	80	124.50	108.73
41	6.61	5.64	81	134.41	117.64
42	7.09	5.94	82	144.86	126.91
43	7.60	6.28	83	155.83	136.48
44	8.13	6.66	84	167.34	146.45
45	8.69	7.08	85	179.37	156.89
46	9.30	7.53	86	191.93	167.91
47	9.94	8.04	87	205.02	179.58
48	10.63	8.59	88	218.63	192.01
49	11.38	9.19	89	232.78	205.28
50	12.20	9.86	90	247.44	219.46
51	13.10	10.59	91	262.64	234.62
52	14.09	11.40	92	278.35	250.84
53	15.17	12.28	93	294.59	268.18
54	16.34	13.24	94	311.35	286.75

* Chances of dying within one year.

For Pennsylvania, values of q_t are available [7] and the proportions p may be computed from the data of the 1930 census. The functions r_f and r^s, r^w are obtained from assumptions (1) and (2) respectively. Approximated mortality rates for married native white males and females in Pennsylvania found by this method are shown in Table II.

Rates of Widowhood and the Effect of Improvement in Mortality Thereon

Age-specific annual rates of widowhood for males and females separately were obtained by computing the weighted means of the mortality rates for the spouse, using the basic data described above. The results are given in Table III and are shown graphically for females only in the left hand panel of Fig. 1.

The rates of widowhood were computed on several mortality bases. In order to bring more sharply into focus the effect of reduction in mortality upon the chances of widow-

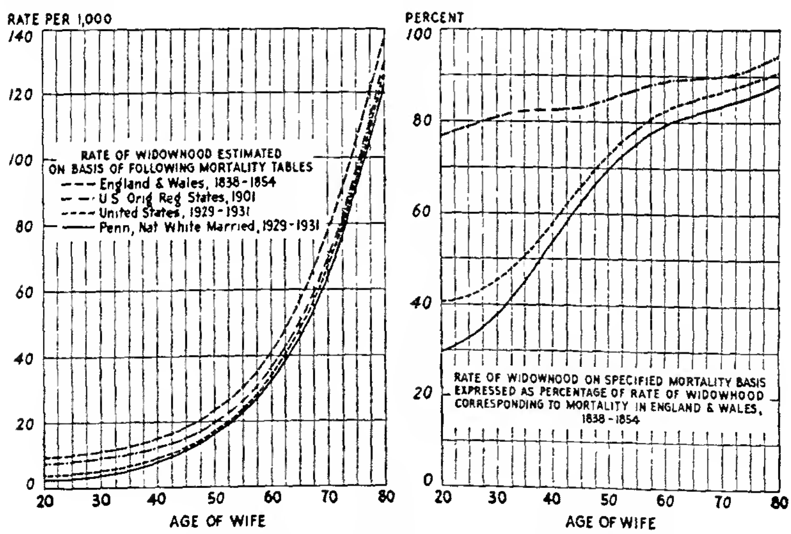

Fig. 1. Effect of improvement in mortality upon rate of widowhood,* native white married females in Pennsylvania, 1930.

* Chances of becoming widowed within one year.

TABLE III

RATES OF WIDOWHOOD PER 1,000,* NATIVE WHITE MARRIED MALES AND FEMALES IN PENNSYLVANIA IN 1930, ESTIMATED ON SEVERAL MORTALITY BASES

Age Group	Mortality Basis			
	Pennsylvania Native White Married, 1929–31	United States White, 1929–31†	United States White, 1901‡	England and Wales, 1838–54
	Males			
Under 20	2.52	2.35	4.82	7.36
20–24	3.16	2.93	5.94	8.72
25–29	3.47	3.34	6.78	9.63
30–34	3.83	3.80	7.62	10.55
35–39	4.46	4.41	8.39	11.53
40–44	5.46	5.39	9.33	12.68
45–49	7.09	6.97	10.82	14.16
50–54	9.60	9.37	13.35	16.31
55–59	13.28	13.04	17.39	20.20
60–64	18.53	18.56	23 13	26.48
65–69	26.84	27.50	32.29	36.68
70–74	38.99	40.45	45.48	51.24
75–79	56.06	58.75	63.87	71.05
80–84	77.88	82.46	87.62	95.64
85–89	102.73	109.60	114.98	123.03
90–94	128.19	137.30	143.17	150.41
	Females			
Under 20	2.56	3.59	6.79	8.91
20–24	2.92	3.89	7.38	9.49
25–29	3.64	4.51	8.31	10.41
30–34	4.80	5.56	9.53	11.68
35–39	6.70	7.38	11 15	13.57
40–44	9.52	10.13	13 48	16.35
45–49	13.48	14.09	17.06	20.42
50–54	19.00	19.75	22.49	26.20
55–59	26.93	28.01	30.66	34.72
60–64	38.44	40 01	42.50	47.67
65–69	54.87	57.01	59.49	66.50
70–74	76.55	79.26	81.99	90.71
75–79	104.99	108 21	112.34	120.90
80–84	138.41	142.28	148.10	155.08
85–89	171.03	175.96	182.54	187.53
90–94	198.14	205.67	210.97	214.37

* Chances of becoming widowed within one year.
† Excluding Texas and South Dakota.
‡ Original Registration States.

TABLE IV

RATES OF WIDOWHOOD PER 1,000 AT EACH AGE FROM 15 TO 84,* NATIVE WHITE MARRIED MALES AND FEMALES IN PENNSYLVANIA, 1929 TO 1931

Age	Males	Females	Age	Males	Females
15	2.52	2.56	50	8.20	15.86
16	2.54	2.59	51	8.70	17.01
17	2.57	2.64	52	9.24	18.25
18	2.61	2.68	53	9.84	19.59
19	2.65	2.73	54	10.49	21.05
20	2.71	2.78	55	11.21	22.65
21	2.77	2.83	56	11.98	24.40
22	2.85	2.88	57	12.83	26.30
23	2.93	2.94	58	13.74	28.35
24	3.01	3.02	59	14.72	30.55
25	3.09	3.11	60	15.77	32.89
26	3.17	3.24	61	16.88	35.38
27	3.25	3.39	62	18.08	38.00
28	3.33	3.57	63	19.37	40.77
29	3.41	3.77	64	20.78	43.67
30	3.53	3.99	65	22.30	46.71
31	3.60	4.24	66	23.96	49.88
32	3.71	4.51	67	25.78	53.23
33	3.83	4.81	68	27.75	56.80
34	3.96	5.14	69	29.90	60.62
35	4.10	5.51	70	32.23	64.74
36	4.25	5.92	71	34.76	69.19
37	4.42	6.36	72	37.49	73.97
38	4.60	6.83	73	40.43	79.05
39	4.79	7.34	74	43.59	84.42
40	5.00	7.87	75	46.96	90.08
41	5.22	8.44	76	50.56	95.99
42	5.46	9.03	77	54.35	102.13
43	5.72	9.67	78	58.34	108.45
44	5.99	10.36	79	62.48	114.93
45	6.29	11.11	80	66.77	121.52
46	6.61	11.93	81	71.19	128.18
47	6.96	12.81	82	75.72	134.88
48	7.34	13.76	83	80.37	141.58
49	7.75	14.78	84	85.13	148.23

* Chances of becoming widowed within one year.

hood, the results obtained on the more recent mortality bases have been expressed as percentages of the rates of widowhood corresponding to the mortality conditions in England and Wales, 1838–1854. The percentages for females are shown graphically in the right hand panel of Fig. 1.

To the extent that the mortality rates for England and Wales in 1838–1854 were representative of conditions in the United States about that time,[12] it may be said that the improvement in mortality in the last century has brought with it a notable decrease in the chances of widowhood for both sexes and at all ages. The decrease is particularly marked at the younger ages and is greater among males than among females. Thus, it will be seen, by a comparison of rates of widowhood corresponding to mortality in the United States, 1929–1931, with those corresponding to mortality in England and Wales, 1838–1854, that for males under age 50 the chances of losing their spouse by death have been reduced by more than half: for females, a benefit of this magnitude extends only up to age 35. An example of the differential in improvement in rates of widowhood between the sexes may be cited from the figures for the age group 30 to 34 years, where it is found that the decrease from 1838–1854 to 1929–1931 is 64.0 per cent for males and 52.4 per cent for females.

If the rates of widowhood corresponding to mortality in the United States as of 1901 are included in the foregoing comparisons, it will be observed that a great part of the reduction in rates of widowhood has been concentrated in the last thirty years. As an example, the rates of widowhood for females of ages 30 to 34 years show an 18.4 per cent reduction from 1838–1854 to 1901 and a 41.7 per cent reduction from 1901 to 1929–1931. From the method by which the rates of widowhood presented here have been obtained, it is obvious that the characteristics noted in the foregoing comparisons according

[12] There is found to be a fairly close agreement in the mortality rates for England and Wales and for white persons in the United States in recent comparable periods.

to age, sex, and time merely reflect corresponding changes in mortality conditions.[13]

Although, of the various results presented above, those for native white married persons in Pennsylvania reflect most closely the conditions in an *actual* situation, since all the basic data relate to this particular population, attention is again called

(1) to the possibility that the estimated rates of widowhood may be too high at the younger ages and too low at the older ages because the mortality table for married persons used in the computation did not take account of duration of marriage;
(2) to the various approximations in obtaining the mortality rates for this case;
(3) to the fact that the table of age distribution of husbands and wives in native white families
   (*a*) deals only with the case where both are present in the family,
   (*b*) was extrapolated beyond certain ages, as previously indicated.

Rates of Widowhood and Mortality Compared

At most ages of married life, a woman has a greater chance of becoming a widow in the course of the year than of losing her own life in the same period. This is to be expected, for in the first place, women, on the average, are married to men older than themselves, and secondly, mortality rates for married men are greater than for married women at most ages of life. However, it will be observed, in the right hand panel of Fig. 2, that between ages 20 and 27, the risk of death within a year for a native white married female in Pennsylvania is greater than the risk of her becoming a widow. It is very likely that this situation arises from the risks associated with maternity at these ages.

[13] In this connection, see chapters 2 and 3 of Dublin and Lotka, *Length of Life—A Study of the Life Table*, Ronald Press, New York, 1936

For males at most ages of married life, the chances of death in the course of the year are greater than the chances of widowhood. In the left hand panel of Fig. 2, it will be seen that among native white married men in Pennsylvania this is the case beginning with age 34: below age 34 the chances of widowhood are greater than of death. According to the curves in Fig. 2, the chances of death for males increase more rapidly with advancing age than the chances of widowhood; among females, on the other hand, the chances of widowhood increase the faster of the two.

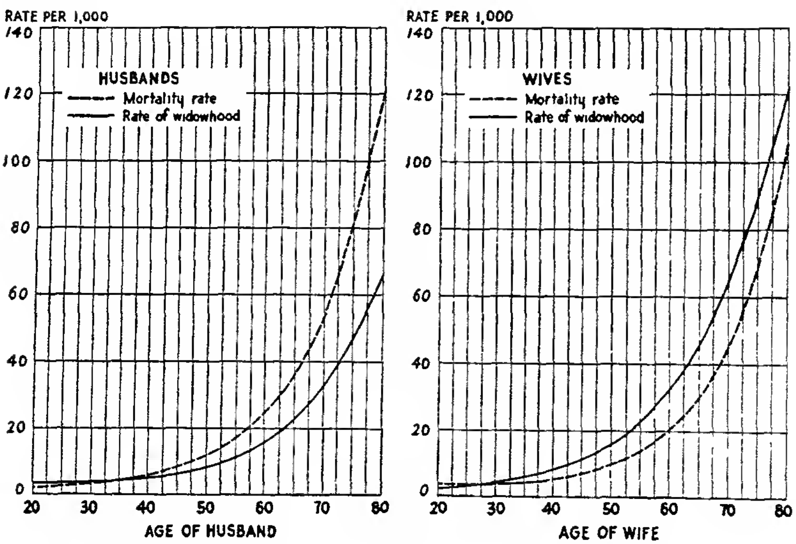

Fig. 2. Rates of widowhood * among native white married persons in Pennsylvania compared with their mortality rates, 1929 to 1931.

* Chances of becoming widowed within one year.

Rates of Widowhood by Alternative Method

Wolfenden also suggested that rates of widowhood could be approximated "if the average age of the husbands of the married women of each age were known." In that case, the rates of widowhood would correspond to the mortality rates of married men at the various average ages. The results of

computations carried out on this basis with the data for native white married males and females in Pennsylvania, are presented in Table V. The second column of this table shows the average age of the spouse (as obtained from Table I) corresponding to husbands or wives of the age groups specified in the first column. The rates of widowhood in the column headed "*A*" were obtained by interpolating the mortality rates in Table II for the average ages shown in the preceding column. For the sake of comparison, there are set forth in column "*B*" the rates of widowhood computed as weighted means of the mortality rates (see second column of Table III).

With the exception of the rates of widowhood for males under age 30, the results produced by the short alternative method, in this case, systematically fall below those yielded by the lengthier method. Since there has already been indicated a possibility that the rates of widowhood computed by the lengthier process may, at the older ages, be an understatement of the true rates of widowhood, it appears that the shorter method, with its still lower rates, gives a definitely biased result at those ages.

Conclusion

The general improvement in mortality which this country has been experiencing since the middle of the last century has brought with it an appreciable decline in the age-specific annual rates of widowhood for married persons, particularly for those at the reproducing ages. Since the decease of either mate terminates a marital relation, premature widowhood operates to prevent the completion of families, and reduces the average size of actual families below that of "completed" families. Any circumstance that diminishes the incidence of widowhood will, therefore, tend to increase, however slightly, the average size of families, and hence to enhance the rate of natural increase of the population, or at least to retard the decline in that increase.

TABLE V

COMPARISON OF RATES OF WIDOWHOOD * FOR NATIVE WHITE MARRIED MALES AND FEMALES IN PENNSYLVANIA, 1930, COMPUTED BY TWO METHODS:
(A) FROM MORTALITY CORRESPONDING TO AVERAGE AGE OF SPOUSE
(B) WEIGHTED MEANS OF MORTALITY RATES OF SPOUSE †

Age Group	Average Age of Spouse	Rates of Widowhood per 1,000	
		A	*B*
	Males		
Under 20	18.49	2.57	2.52
20–24	22.12	3.41	3.16
25–29	25.75	3.48	3.47
30–34	30.19	3.73	3.83
35–39	34.78	4.33	4.46
40–44	39.39	5.24	5.46
45–49	44.05	6.68	7.09
50–54	48.69	9.00	9.60
55–59	53.17	12.44	13.28
60–64	57.60	17.31	18.53
65–69	62.34	24.60	26.84
70–74	66.84	35.19	38.99
75–79	71.09	51.35	56.06
80–84	74.98	71.52	77.88
85–89	78.39	95.37	102.73
90–94	81.27	120.14	128.19
	Females		
Under 20	24.07	2.54	2.56
20–24	26.90	2.86	2.92
25–29	31.18	3.45	3.64
30–34	35.76	4.51	4.80
35–39	40.60	6.43	6.70
40–44	45.56	9.03	9.52
45–49	50.52	12.67	13.48
50–54	55.28	17.98	19.00
55–59	59.97	25.25	26.93
60–64	64.65	36.19	38.44
65–69	69.23	51.73	54.87
70–74	73.44	72.82	76.55
75–79	77.38	101.11	104.99
80–84	80.88	133.22	138.41
85–89	83.54	162.05	171.03
90–94	85.98	191.68	198.14

* Chances of becoming widowed within one year.

† The weighting used is the relative frequency distribution of the spouse among wives (or husbands) of a specified age group; see second column of Table III.

Even more important than this relatively slight effect of diminishing widowhood are the direct and concrete benefits to family life. Not only is the community's burden for dependent children reduced, but the benefits of parental care are made available to children who, under the regime of earlier mortalities, would have been deprived of that advantage.[14]

[14] A. J. Lotka, "Orphanhood in Relation to Demographic Factors," *Metron*, 9, No. 2 (August 1931), p. 37.

TECHNOLOGICAL ADVANCE IN RELATION TO POPULATION TRENDS

Discussion

WALDEMAR B. KAEMPFFERT

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(*Read November 18, 1938, in Symposium on Population Growth*)

The republic is less than 200 years old. During its brief career a wilderness measuring millions of square miles has been transformed into an agricultural and industrial empire. The story of this rapid transformation is old now, a story of frontiersmen who pushed out from the Atlantic seaboard until at last they reached the Pacific, hewing farms out of the forest, planting tobacco and cotton in the south and grain in the middle west, launching steamboats and building railways, mining about all the metals that an industrial nation needed.

Expansion—Characteristic of the Past

Incessant expansion characterized this conquest of the wilderness—the occupation of new land, the tapping of new resources, the hunger for more personal and national wealth. Today the choice acres are settled, the best stands of timber cut, the richest mines and oilfields discovered, with many yielding less than they did twenty years ago. Physical expansion is no longer possible.

Technology conquered the old frontier—the rifle and trap, the mechanical reaper that dispelled the dread of starvation, the heavy freight locomotive and the 70-car train so vitally necessary in developing the wheat lands of the middle and far west, the labor-saving sawmill, the cot-

ton-gin that changed the whole social and industrial character of the south, the barbed wire fence that simplified ranching. Labor, more labor and still more labor was the demand for generations. Because it could not be stilled by even an abnormal increase of population—that is the normal increase plus immigration—we have been the great inventors of machines for lifting, bending, twisting, breaking, digging, boring, conveying, communicating. Our more acute labor troubles began only after the good land had been settled and we restricted immigration. The resultant rising wages brought in their train more labor-saving devices to meet competition in domestic and world markets.

Coming Stability of Population

In the past expansion was possible only with a growing population. That growth was assured by a high rate of reproduction and by immigration stimulated by the offer of political liberty, grants of free land and economic opportunity. All this is changed. It is now established that in the not too distant future the population will be stable; after which it will decline. Let Warren S. Thompson and O. K. Whelpton present the results of their latest studies:

> On the assumption of a medium fertility and mortality rate, the population will continue to grow for 50 years, but at a constantly decreasing rate, reaching 153,000,000 in 1980. Assuming a net immigration of 100,000 persons per year from 1940 on, the figure for 1980 is raised to 158,000,000. Even with the highest rates that can be reasonably assumed, there would be a natural increase of less than 50,000,000 from 1935 to 1980. The minimum estimate assumes a decline of about one-third in the fertility of native white women from the 1930–1934 level to 1980, with no net accession of immigrants. This estimate gives a peak population of 138,000,000 in 1955 with a decrease of 10,000,000 in the next quarter century.

It must not be inferred from this preview of our future population that we may cease to worry about unemployment and that the workless man of forty and over will no longer perplex the government and the business man. If

not a child were born in the United States in the next fifteen years there would still be unemployment. Newly born children do not crowd the labor market. Indeed Dr. R. R. Kucynski goes so far as to say in his *Population Movements* that "if one were to set out to increase unemployment in a given country for the next fifteen years, one could not adopt more efficient means than birth control on a very large scale." He points out that with fewer children coming into the world, many teachers, governesses, physicians, builders of schoolhouses, makers of toys and infants' clothes, publishers of schoolbooks, manufacturers of perambulators, roller skates and baseballs would have to find work in new callings. In fact every industry and profession would be affected.

Not until the mean expectancy of life at birth increases greatly, not until we learn how to control cancer and the degenerative diseases—failure of the heart, kidneys and other organs—will the average age of the working population be so high that industry will find it impossible to recruit employees entirely from the class that is under 40. And that may not be until 2050, so far as the present rate of medical progress indicates.

No Decline in Industry in Sight

Because the population will decline it does not follow that industry will decline with it. Human wants grow as they are satisfied. The luxuries of the old frontier are bare necessities now. Today a "cold water flat" in New York is a substandard abode, so accustomed are we to both hot and cold running water. Possibly by 1980 a television set which will bring opera and plays into every home will be as essential as hot water or a supply of gas.

In a paper which he read before the American Association for the Advancement of Science in December, 1937, Mr. Frederick Osborn made the point that economic growth need not cease with stabilization of population. Despite

our remarkable productivity there is no reason to believe that wants will not continue to expand, whatever the demographic outlook may be. Mr. Osborn indicates that in rural areas alone there are about 20,000,000 whose standard of living is so low that they play no active part in the economic development of this country. Remove these from the scene over night, and they would not be economically missed. The national income would not be affected; commerce and industry would be little the worse off. But suppose these 20,000,000 together with the unproductive in our cities could be made to produce and hence to consume. The effect would be startling. It has been estimated that if the millions who must now remain ill-fed were to eat as well as the upper twenty-five per cent we would have no exportable surplus in 1980, by which time our population will have reached its peak.

Paradoxical as it may seem, much invention has been stimulated by the unions. It was they who insisted on the passage of immigration laws which made it more difficult to recruit cheap European labor for trench-digging or shoveling ore in steel mills or doing the manual work of the mill and the mine. The result is that when an oil or gas line is to be laid hundreds of miles a trench-digger now does most of the work—a colossus that gnaws its way from one end of a state to the other.

There were steam shovels before the major restrictions on immigration were imposed, but not the Titans now busy on the Mesaba range, where iron is dug up at the surface like so much dirt. We had labor-saving machines when wages were far lower than they are now. The point is that when wages go up it becomes possible, even necessary from a business angle, to invent machines of a new type and of unprecedented productivity.

When, therefore, a manufacturer protests against demands for higher wages or shorter hours and vows that he must either close or move to non-union territory, or when a financier decides that he will not invest his money

in an industry because of high labor costs and small profits, he assumes that production costs cannot be reduced, that the inventors cannot meet the exigencies of a new situation.

In the decade from 1920 to 1930, one of steadily rising wages, the nation's output increased 46 per cent, but the labor force only 16 per cent. It would be fallacious to attribute this remarkable decline in opportunities entirely to new and more complicated inventions. Still Mr. David Weintraub, a close student of technological trends, finds "definite meaning" in the percentage.

That there is indeed "definite meaning" in these percentages becomes apparent when we see their effect on employment policies. With skill and intelligence transferred to the machine there is a glut of labor on the market. It becomes possible for employers to select labor. There is little doubt that as the result of selection, middle age is economic old age, that industry is creating a human scrap heap, that business in hair dyes must be good.

Case of the Man Over Forty

The WPA records give striking evidence of discrimination against the man over 40. Out of 416,082 workers on relief in 13 cities during the last quarter of 1935 an average of 2.5 per cent found employment each month, or 10,088. But it was the men between 25 and 34 who were "separated" from relief, as the specialists say—4.1 per cent, or 3,431 out of 84,437. Of the group 35–44 years old 3.1 per cent got jobs—3,308 out of 106,523. Evidently even 35 seemed old to many employers. Only 1.9 per cent of the men between 45 and 54 were successful—1,739 out of 92,779.

He is no sudden apparition—this workless man over 40. As far back as 1866 witnesses testified before a special Massachusetts Commission that wood carvers and cabinet workers were economically old after 40. Judge Altgeld, reviewing the arguments for the eight-hour day in 1890,

wrote that at 35 or 40 men "break down in the wake of exhausting toil and exposure and at 40 too many of them are in their graves." The New York Commissioner of Labor Statistics in 1900 set the deadline for women at 45 and men at 50 and before the Industrial Commission pleaded for the man "whose hair is gray but who is physically a strong man and looks to be in the prime of life."

Position of Old Craftsmen

Before the machine age the craftsman of 40, 50 or 60 had an assured position. His guild was a tyrannical hierarchy, a closed shop. The young were apprentices for a fixed period; the old, masters and teachers. So it happened that the distribution of young and old in the crafts was much the same as in the general population. Manufacturing—spinning and weaving, for example—was a cottage family enterprise in which the comparative productivity of grandfather or grandson was not considered. It was enough that there were no idle hands. Whether a man worked at a bench as a craftsman or did piece work at home he owned his tools and ordered his life as it pleased him. His major concern was to get a fair price for his work.

The modern factory with its emphasis on efficiency, subdivision of labor, mass production, created the problem of the jobless propertyless man over 40.

The Unskilled Suffer

The more complex the mechanism the higher the skill demanded to operate it. But it is a kind of skill which has little in common with the old craftsmanship. On the whole the men in charge of these mechanical Titans are fairly secure in the tenure of their employment, though they, too, have innovations to fear. It is the unskilled and the semiskilled who have the greatest reason to shudder at the

specter of middle and old age. They are the principal actors in the tragedy of the machine.

Yet contrary to popular supposition, the proportion of those between 40 and 64 designated by the census as "gainfully employed" (that is the number who usually follow an occupation) is greater in normal times today than it ever was before. It constituted 28 per cent of the population in 1900, and 34 per cent in 1930.

But this favorable showing is accounted for partly by the influx of women into industry—the typists, the telephone operators, the women who manipulate adding and bookkeeping machines in offices and who feed material to automatic machines in factories. And also by the elimination of children. In 1890, 18 per cent of them between the ages of 10 and 15 were gainfully employed; in 1930 the census reported only 5 per cent. The reason is to be found in the legal restrictions governing employment in most states and in compulsory education.

Drs. Ralph G. Hurlin of the Russell Sage Foundation and Meredith B. Givens of the Social Science Research Council have broken down the census figures since the Civil War for various occupations and shown that the proportion of men employed in manufacturing between 45 and 65 in 1930 was much as it was in 1890. But the productivity of a man at a machine has increased enormously.

An example: In 1889 a total of 575 blast furnaces produced pig iron and ferro-alloys in the United States. The total capacity of the furnaces was 13,168,000 tons of pig iron and ferro-alloys annually.

In 1938 the number of blast furnaces was only 236. But their total capacity was 51,221,000 tons a year.

Since 1889 the average capacity of blast furnaces has increased about 700 per cent.

Reasons for Discrimination

Why does industry discriminate against the man over 40?

"A bad physical risk" is the first charge. The man over 40 is entering a period when degenerative diseases will lower his efficiency or incapacitate him entirely, it is said. But the statistics show that carpenters, bricklayers, plumbers, printers, compositors, tailors, garment workers, garage mechanics, white-collar workers are about as healthy as the rest of the population and die at the normal age. But sand blasters, axe grinders, men in plants where clouds of steam are liberated at times have a high mortality rate. On the whole the highest death rate is found among the unskilled and semi-skilled. The only exception is to be found among farmhands. They come right after the white-collar workers. So the charge "bad physical risk" is true of some occupations but not of others.

"Lowered productivity" is the second of industry's charges against the man over 40.

Few direct measurements are available. There is no denying that the efficiency of the class between 40 and 65 as a whole is below that of the class between 30 and 40. But the difference is not so great as industry supposes. No precipitous decline occurs. In fact it is slight until 55 is reached according to the measurements available. After that the drop is steeper. A deadline is therefore not justified.

"Bad accident risk" reads industry's third charge against the man over 40. Industry is wrong. In a survey which differentiated between jobs held by the young and the old the British Industrial Health Research Board found the man over 40 more careful than his younger competitor. Experience and age have made him so. The British findings were confirmed in a study of 65 manufacturing plants and four railroad repair shops in New York state.

On the other hand the man over 40 does not recover so rapidly as the younger man from an injury. Accident compensation enters here to aggravate the plight of the man over 40. His recovery is expensive.

"Too slow and inadaptable to changing conditions" is the fourth charge against the man over 40. Perhaps it is true.

It is not the worker who sets the pace in a modern factory but the whole technique of production. The tendency, then, is to weed out the slow. As the machine is improved to do more work with less attendance the pace quickens.

Whichever way governments, employers and workers turn they are baffled. Payrolls are taxed, as under Federal and State Social Security Acts, with the inevitable result that more machines must be invented to replace more men.

Old-age pension systems and group-insurance schemes are adopted which prevent the hiring of the man over 40 because he reaches the retiring age too soon or because his forty or more years bring up the average of the group and hence the rate that must be paid. When they can, unions stipulate that promotion shall be by seniority or that a certain number of older men shall be employed in closed shops, only to rob ambitious youth of its opportunity. Sharing the work is urged but at the expense of the standard of living and with the certainty of incurring worse labor troubles than we have.

Out of every thousand persons in this country 260 are 40 or more years old. By 1960 statisticians estimate that 36 per cent of us will be 40 to 65 years old. Because of a declining birth rate, and a declining infant mortality, because of a longer expectancy of life at birth, because of our betterment of the public health, we are rapidly becoming a nation of elders. Is the extension of human life to be an extension of misery for millions? Years have been added to life. But what kind of a life have the added years brought with them?

It is not the purpose of this article to offer a solution of the problem but to picture a tragedy. Whichever way govments, employers and workers turn they are baffled. Payrolls are taxed, as under Federal and State Social Se-

curity Acts, with the inevitable result that more machines must be invented to displace more men.

Whatever the advantages may be, the five-day week is no remedy in view of the historic fact that the steady reduction of the working day from fourteen or sixteen hours to eight in the course of a century has not stabilized employment. Foreign trade offers no hope in these days of tariff walls and barriers. Social-security acts conform with traditional commercial insurance practices and fail to meet immediate needs because they inevitably shunt the idle worker to the relief rolls after the statutory period of assistance has expired.

It is hard to imagine the United States, richest of all countries, a leader in technological advance, abdicating in the face of the obvious necessities. We need a comprehensive program of social insurance, embracing unemployment insurance, old-age pensions or insurance, a tax-supported medical system or health insurance with both medical and cash benefits and a system of family allowances which will eliminate child labor. We have only scratched the problem of the man over 40 with our Federal and State Security Acts.

PROSPECTIVE DEVELOPMENT OF CULTURAL PATTERNS IN RURAL AMERICA AND THEIR POSSIBLE INFLUENCE ON POPULATION TRENDS

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(*Read November 18, 1938, in Symposium on Population Growth*)

In his paper, Dr. Taeuber presented a clear characterization of farm population trends and status. The broad outlines of his characterization were: large population naturally increases in farm areas, but between 1910 and 1930 a declining farm population; the highest fertility ratios of the nation in the low income farm areas, and the lowest farm population fertility ratios in the high income farm areas; a pronounced cityward drift of farm population for at least four decades. He points out a number of modifications and some exceptions but substantiates these as sound generalizations. Baker, Lorimer and Osborn, Thompson and Whelpton, Woofter, Vance, and others also have contributed exact data to substantiate the soundness of these conclusions. Recently Dr. Ray Mangus, in an attempt to delineate the rural cultural areas of the nation, found a striking correlation between areas in which the rural farm population had low levels of living and high fertility ratios, and between areas of high levels of living and low fertility ratios. When the counties of the nation were arrayed on the basis of each of these two indexes and the results placed graphically on maps, he found a striking identity between areas with low planes of living and those with high ratios of fertility. He found the same exceptions mentioned by Dr. Taeuber; namely, an area including Utah and parts of Idaho with high planes of living and also high fertility ratios, and the Mississippi Delta with low planes of living and relatively low fertility ratios.

Dr. Mangus and Dr. Taeuber alike delineate the outstanding areas of low standard of living and high fertility ratios as the Southern Appalachian and Ozark Highlands, parts of the Old South, the Indian—Spanish-American areas of the Southwest and the Pine Ridge Indian reservation of South Dakota. The areas of high planes of living and low fertility ratios are the Eastern Seaboard manufacturing areas, central New England, central New York, the Middle West, California, the Puget Sound area, the Columbia-Willamette Valley, and the plateau wheat country of Washington and Oregon. For the sake of simplifying consideration of these different types of areas, I shall eliminate the exceptions mentioned above; namely, the Mormon area and the Mississippi Delta, eliminate the Indian and Spanish-American areas because of their unique types of culture, and discuss only areas of high planes of living and low fertility ratios.

As a text for my contribution let me quote one sentence from Dr. Taeuber's conclusions, viz., "Large families were part of the culture complex which included a predominantly self-sufficient agriculture; smaller families appear to be a part of the culture complex which includes a predominantly commercial agriculture."

In attempting to set forth patterns of rural culture which may develop and prevail in the future, we can probably do no better than to use these two modal types, especially since they do seem to have definite cores as shown by such empirical studies as I have just cited. They represent, of course, the extreme poles in a considerable array of rural cultural types in this nation. We shall designate them for purposes of this paper as the self-sufficing, subsistence, noncommercial or peasant type of culture, in which the material plane of living is low, annual cash income is relatively low, and fertility ratios are high, and the commercialized or rationalized, mechanized, urbanized type of culture, in which the material plane of living is high, the annual cash income relatively high, and the fer-

tility ratios low. Between these two extremes fall many intermediate types and some exceptions such as were mentioned above.

Let us, for purposes of isolating these extreme types of culture, classify as self-sufficient or noncommercial all farms which had a total value of farm products of less than \$600 each year and as commercial farms all those which had a total value of products of \$2500 and over. When this is done, we find that there were in 1930 1,682,000 noncommercial and 1,153,000 commercial farms, or 26.7 per cent in the first group and 18.3 per cent in the second group, with the remaining 55 per cent ranging between these two. The noncommercial farms, as defined here, were concentrated in the Appalachian-Ozark Highlands, were scattered throughout the Cotton Belt, appeared in the same Indian and Spanish-American areas mentioned by Dr. Mangus, and were well represented in the Lake States Cut-Over Area. The commercial farms, as defined here, appeared in greatest numbers throughout the Corn, Dairy, and Wheat Belts, in the valleys of the far western States, and to some extent in the eastern shore manufacturing areas, central New England, and central New York. They are almost identical with the location of the two extreme types of culture discussed by Dr. Mangus.

Two things should be clearly understood at this point: first, that the type of farm described here is not identical with, although it includes for the most part, the "self-sufficing" farm as defined by the census; and second, that neither the "self-sufficing" farm of census definition nor the noncommercial farm described here necessarily produces a larger amount of home-consumed goods than many commercial farms. Hundreds of thousands of farms classified here as commercial yield to their occupying families a far greater volume of consumable goods than do the so-callèd self-sufficient farms, and what few data are available indicate that the great mass of farms of the nation which are intermediate between these two extreme types, namely,

those that are most diversified in relation to various types of crop and animal production, are in fact the greatest producers of home-consumed goods. It should also be pointed out that so-called self-sufficing farmers in the great majority of cases earn money from off-farm work.

The point I am trying to make, however, will be almost completely lost if it is assumed that what I have called the self-sufficient or noncommercial type of rural culture is indexed solely by the proportion of farm products that is consumed by the farm family, or that the commercialized or rationalized type of farm culture is not a living reality merely because it yields to its operating families a greater amount of home-consumed goods than is the case of the "self-sufficing" farm as defined by the census. My point is that the major orientations of day by day life are different, the techniques of production and business operation are considerably different, and even the thoughts and purposes are different in a culture which is highly rationalized and the major orientation of which is with forces which lie beyond the farm and without the farm community from those of a rural culture the major orientation of which is to the farm, the farm home, and the local community. In one case there exists a passionate drive for higher cash incomes, higher standards of living, and excessive mobility in search of opportunities, while in the other such drives, if existent, run at low tide and stability dominates both behavior and ideologies.

It is not my purpose to follow in any detail those areas which, according to one or another statistical classification, may be called commercial or noncommercial. Rather I shall describe briefly the general characteristics of these two patterns of culture, both of which exist in wide areas of this nation. Any attempt to do so leads to the unquestioned conclusion that we have moved steadily from a relatively self-sufficient type of agriculture toward a relatively commercialized type of agriculture. Under the prevailing economy of self-sufficiency of 100 or 200 years ago, the

local neighborhood, even the local farm, produced practically all of the goods consumed by the farm family. As far as farm production was concerned, the chief objective was to accumulate during the planting, cultivating, and harvesting seasons enough commodities to supply the farm family throughout the cycle of the year. Very little was purchased from the open market and very little was sold through the channels of trade. Practically all of the work was done by the farm family, and communication with others than the family was practically altogether by word of mouth. There was therefore a highly integrated family and neighborhood life. World events transpired days, weeks, and months before knowledge of them reached local communities.

Due to the relatively high degree of isolation and absence of market contacts, the rural material standard of living consisted almost altogether of those things which could be and were produced on the farm or in the local neighborhood. Housing was simple and sometimes poor; illiteracy was common; and both death rates and birth rates were high. There were no radios, no running water or electricity in the homes, and no automobiles or telephones. The type of home, neighborhood, and community life was not strikingly different from that which prevails in the self-sufficient farming areas of the nation today. The difference was that the vast majority of farm families lived in this type of culture 200 years ago, whereas today only a minority of them does. This minority, however, includes a good many hundred thousand farm families and these farm families and their ways of life dominate wide areas in some sections of the nation.

The change from a rural society based largely on "self-sufficing" farming to one dominated largely by commercial farming has come about by a gradual but quite consistent evolution. The penetration of what Veblen called the "price and market regime" has, within the last 75 years, gone so far in American agriculture as to convert what was

at one time only an occupation into what is today a business enterprise. Most farmers are today more concerned about producing products for sale than they are about producing them for their own consumption. Millions of dollars are spent annually in the purchase of machinery, production materials, and even family consumption goods. Even the worth of the land itself is measured in dollar values per acre, and farm mortgage debt is common and to be expected as a part of the financial structure of farming.

The most outstanding factors that have entered agriculture to change the type of daily life which existed on the farm in the past are mechanization of production, the improvement in means of transportation and communication, and the production for market sales. The introduction of these things, plus the development of scientific farming, has considerably changed the ideologies of rural people, and it is without question true that attitudes and values highly cherished by rural people have given way under the impact of these evolutionary changes. Those engaged in commercial agriculture today apply less human labor per unit of farm product than in the past. The increased application of science to agriculture has brought to rural people a much more exact knowledge not only of their day by day problems, but of the world at large. There has developed within American agriculture the highest material standard of living known by any farm people in the world, and with this high standard of living, as has been pointed out by Dr. Taeuber and Dr. Mangus, there has almost universally followed a decline in the birth rate. Less obvious, but just as true, has been a considerable sacrifice of the cohesiveness of family life, a considerable loss in the homogeneity of rural community life, a depletion of folk culture, and a high degree of insecurity on the part of millions of farm families. In fact, it is only among groups which have, even with increasing technological development and higher levels of living, retained in considerable degree their group integration, homogeneity, and psycho-

logical orientation relative to a set of values in life that the decline in the birth rate or rate of reproduction is relatively slight. Examples of such groups are the Mormons, and smaller communities dominated by single ethnic or nationality groups in which the original culture is to a considerable extent retained.

One further outstanding population trend not discussed by Dr. Taeuber needs to be briefly discussed before we turn to speculation concerning the future. The shift from farm to non-farm employment has traveled with accelerated speed for the past 50 years. The percentage of those gainfully employed in agriculture has diminished every decade from 1870 to 1930. All other occupations and professions had in 1910 gained over 1870, but between 1910 and 1930, the percentage of the total engaged in forestry and fishing and in mining diminished; the percentage in manufacturing and mechanical industries increased only very slightly; those engaged in domestic and personal services remained about the same; and all other occupations except agriculture increased. The greatest relative increase has been in the clerical occupations, next in trade, followed in order by professional services, transportation, and public services. In gross numbers, between 1910 and 1930, the gainfully employed in trade and transportation increased 171,973 more than in manufacturing and mechanical pursuits, and in agriculture there was a decrease of 1,119,769 during this same period.

An analysis of trends in migration indicates that persons move out of given rural areas to other rural areas, or move cityward, because they are, so to speak, pushed out of old locations and pulled by more promising alternative opportunities. The recent well and widely known tendency of population to pile up on poor lands, and the fairly well demonstrated tendency of good farm areas to be equally migrative with poor land areas, lend proof to the conclusion that it is the pull of alternative opportunities that constitutes the chief cause of migrations. Some ob-

vious evidences of this fact in American rural migrations are: The increase in farms and rural population during the recent depression were in the poor land and rural poverty areas. There was an increase of 523,000 farms, or 8 per cent, between 1930 and 1935. The increase in the sixty-six counties of the nation, in which in 1930 fifty per cent or more of all farms were "self-sufficing," was 33,305 farms, or 23.84 per cent. These sixty-six counties contained only 2.1 per cent of all farms in 1930, but furnished 6.3 per cent of the increase in farms between 1930 and 1935. These new farms were for the most part small, "self-sufficing," or part-time farms located in poor land areas. Not only from 1930 to 1935, but also before 1930, the loss in rural population by migration was not confined to poor land areas. There were considerable losses by migration in some of the best land areas, where standards of living were high and where because of higher levels of educational attainment and a greater knowledge of alternative opportunities, the pull outward served to stimulate mobility.

It is thus apparent that by and large rural-urban migrations depend on the comparative economic status of agriculture and other occupations and professions. If, therefore, there is evidence that other occupations and professions will again outbid agriculture for the working force of the nation, this will almost automatically guarantee the continuance of cityward migration in the future. It will not guarantee the rapid depopulation of the poverty and high birth rate rural areas to any greater extent than it will the richer agricultural areas, and thus there will continue to be wide differences between the amount of the nation's population produced by the good and poor farming areas, unless other cultural factors become more influential than poverty in influencing the birth rate.

Thus the line of demarcation between the producing farmers on the one hand, and the reproducing farmers on the other hand, will become gradually more marked. It is

when we look at the almost certainly resulting concomitants of this situation that we have the audacity to suggest that alternatives to such a situation may develop, and the trend in population slacken and even change direction.

Thus far the discussion has been largely confined to conditions and trends as they prevailed before 1930. Since 1930, there have been such marked changes as to raise a number of questions. What will be the trends in rural culture in the future? Shall we continue in the direction of greater rationalization in agriculture? Will the movement of farm population to urban centers again reach tides equal to those of the twenties? Will the occupations such as trade, transportation, and the services continue to absorb an ever-increasing percentage of the gainfully employed? Or will legislative action providing for Social Security, Farm Security, and land adjustment programs alter the scales of economic competition; will international complications alter our economic and social life to such an extent as to sharply change our frontier and isolationist ideologies; and is there even a possibility that there are within some present trends, forces and factors which will alter the direction of these trends in the future?

It is patent to say that if we knew the answers to all these questions and others, we could possibly predict the population trends of the future. But we do not know these answers, and therefore cannot predict surely; and I am definitely of the opinion that merely to project into the future present rural population trends, whether in the fields of vital statistics or migrations, and to assume that these trends will continue unabated and unmodified, is pretty naive. And since the only sure conclusion I have concerning the difficult matter with which I am wrestling, is that no sure predictions can be made, I shall have to content myself with speculation on alternative possibilities, and the limits of both space and intelligence dictate that not all possible alternatives be discussed. I have there-

fore selected those which seem to me most likely to be operative. They are not and cannot be exclusive of each other, and no one of them alone, unmodified by others, will probably dominate future trends.

First let us assume that the major trends which had prevailed for more or less long periods of time before 1930 will be continued or resumed in the future. This will mean that the urbanward migration will again absorb the major portion of rural population natural increase; that the good agricultural areas will lose population relatively, and in some cases absolutely, more rapidly than the poor agricultural areas; that the differential in birth rates between the urban and rural populations and between the good and poor agricultural areas will continue; that the enterprise of agriculture will employ a steadily diminishing percentage of the gainfully employed, that manufacturing and mechanical pursuits will employ about the same percentage, and trade, transportation, and professional services will employ a steadily increasing percentage; and that commercial agriculture will continue to gain ground at the expense of self-sufficient or noncommercial agriculture. This is accepted, tacitly or otherwise, by a great many persons. For them, future populations, like the stars, are fixed in their orbits and they have little hesitancy about predicting that population trends will continue very much as they are now behaving.

Even if all of these trends do in the main continue, they may not be unabated and unmodified, and population trends may not follow faithfully the patterns which have evolved in industrial urban culture. There is a possibility that trends in commercialization, mechanization, and urbanization may outrun themselves in the sense that they develop, as a part of their sum total technique, phenomena and practices which will tend to thwart their steady onward march.

If trends do continue as they have been in the past, then farms in the better land areas will increase in size, use of

farm machinery will become more prevalent and widespread; consequently, the number of farm families in the better land areas will be fewer and the amount of labor required will be less. Under normal price levels and wage and interest rates, farms in these areas will become more profitable, net incomes per farm and family will be higher, levels of living will be higher, urbanization will increase, and birth rates will fall. Furthermore, any population in excess of that needed to operate the farms will, as in the past, tend to migrate out of these areas and, as for the past 40 years, move into towns and cities and thus depopulation continue to a point where the culture of the best farming areas will become very similar to urban culture.

In the poorer land areas, unless they are zoned against certain types of occupancy, small farms will tend to predominate, incomes will remain low, mechanization will advance slowly if at all because of the type and magnitude of the farm enterprise, birth rates will continue considerably above the national level, and although there will be considerable migration out during periods of prosperity, there will be considerable in-migration in periods of depression.

If it should be true, as many believe it will be, that mechanization in industry will reduce or at least increase only slightly the demand for urban workers and thus stop or impede greatly the flow of farm migrants to cities, and if high birth rates continue in the poorest farm areas, there will be but one result; namely, that population will pile up on poor lands to such an extent that public action of one or many kinds will be initiated to alleviate the situation.

It is doubtful that such public action will over a long period of time continue to be chiefly that of direct relief or even continual subsidies to the populations in these overpopulated areas. Alternative adjustments to such direct subsidizing measures might well include stimulated and guided migration to the better land areas, possibly a de-

centralized, industrialized movement, and almost certainly social security legislation, the provisions of which would stimulate if not require changes in occupation and thus changes of residences of the people living in such areas. Under the impact of such a possible set of activities or programs, there will undoubtedly be a continued tendency toward cross-fertilization of the different cultural areas through the process of a continuous flow from areas of high birth rates to areas of low birth rates, and there will be operating in areas of high birth rates, agencies and processes which tend to change the characteristics of the culture which has induced high birth rates. In any case, the sharp differences between birth rates in the various rural areas, and between the rural and urban areas will probably gradually diminish through time.

Some analysis of the occupations into which the gainfully employed have flowed during recent decades reveals a trend which I think it is highly improbable will be continued. As stated before, trade and transportation, professional and domestic services, and clerical occupations have increasingly absorbed greater proportions of the gainfully employed, and as far as this shift to trade and transportation is concerned, it means an ever-increasing cost between the producer and consumer. This trend cannot continue forever. Indeed it is doubtful if it can continue with its present acceleration for any considerable period, for if it does, the percentage of gainfully employed in agriculture would within the next generation approach zero, and the percentage of gainfully employed in trade and transportation would during this same period approach fifty.

There is already evidence that transportation and other distribution costs are so high that a large proportion of the southern farm population is not purchasing wheat flour, dairy products, and fruit from those areas which live by the production of these products for cash sales. Economic costs of distribution have become so high as to destroy the

capacity of those not living in the dairy, poultry, fruit, vegetable, and even wheat areas to purchase any large quantity of their daily supplies of food products from other areas. If this tendency continues, and it can't be escaped if more and more persons must be supported in these middle processes, how long will it be before specialized farmers will begin to produce a greater amount and diversity for home consumption? Should this happen, then diversified, balanced or real self-sufficient farming will increase rather than diminish; the national, historic trend toward ever-increasing commercialization of farming will be modified; and possibly the culture of self-sufficient farming, a part of which is its population pattern and trends, may be somewhat revived.

But let us assume something quite different, namely, that the trends preceding 1930 arose out of a course of events which will probably never again be found in the same combination that existed all during the period of our agricultural and industrial pioneering and expansion; that in fact our whole national development has been so atypical and probably abnormal that little, if anything, that was typical of the past will continue in the future; that neither agricultural nor industrial expansion of either the magnitude or diversity of the past can or will be true of the future, and that our major impulses and strivings of the future will be for security and therefore for stability rather than for speculation and mobility.

Under these circumstances and conditions, the ownership of, or permanent tenure on, the land will probably become a major passion of hundreds of thousands of people who do not see within their purview any very great diversity of alternative opportunities. Conservation, rather than exploitation, will under these conditions, tend to become not only the technique, but the mode of thinking of agricultural people. This may go so far as to lead to the salvaging of certain land areas which, under the impulses

of exploitation and expansion, have been and are being wasted, and thus make habitable areas now seemingly marked by destiny for depopulation.

Should this occur, it would turn the tide of events in the direction of a relatively less commercialized agriculture. American farm people, like the peasants in some of the older civilizations, would probably reëstablish a greater control over their own destinies by producing the maximum quantity of home consumable goods, render a greater diversity of technical and social services to themselves and neighbors, refuse to mortgage their homesteads, and thus create more highly integrated culture communities than is the rule at the present time. Many of these communities might be "rural-industrial" communities. Under these circumstances, even the material elements in the standard of living will be less indicative of their concerns and drives, and almost automatically family and community life will be more indicative. The results of an evolution of this type will probably be less migration from place to place, including less migrations from the country to the city, a tendency to keep the population natural increase within both the upper and lower bounds of a stabilized community, and considerably less tendency to react directly to what we now quite commonly call urban influences.

Another course of events, of an international character, may be emerging which will sharply alter our national psychology and in due time alter our population trends. If it becomes necessary in the near future, or within the next generation, to develop a national unity and cohesiveness as a protection against situations which are or seem to be developing in other areas of the world, we may have not only the development of giant munitions and war preparedness industries, but also the development of a national consciousness which will tend to restrict our insatiable passion for higher material standards of living, and may substitute for that passion a desire to become a strong, virile,

and populous nation. Under such a nationalist psychology, the failure to have children and the individual striving for personal economic advancement may very well become taboo, just as they apparently have to a considerable extent in the totalitarian states of Europe.

Whether a society or any segment of it can be induced to increase or hold up its birth rates by propaganda measures is yet an open question, but such a possibility should not be wholly eliminated, especially if the movement be cast on an indirect and cultural basis. If, for instance, great segments of our population become convinced that city occupations are going to be overcrowded and that city life is pretty insecure, or become weary in the ceaseless and unrelenting struggle for commercial, economic footing, and if they seek the shelter of some more secure mode of living, this security may be found by remaining on the land or by the return of a good many farm-born persons to farm life. Or if the birth rate continues downward in cities, sooner or later the demand for rural-born persons to fill job vacancies created by urban deaths will undoubtedly greatly increase and more rural persons will migrate to cities.

Sooner or later, however, the economic and social injustice of taxing the rural poverty areas of the nation with the economic, social, and biological costs of bearing, rearing, and educating the working force will stimulate the development of health, sanitation, educational, and even recreational agencies, supported by taxes from other than local sources. Once these agencies are established they will change the whole culture of these areas and the birth rates will probably fall. Or if social security programs are extended so as to include more completely farmers, as there is every likelihood that they will be, then security will be found in a rationalized rather than a folk type of society, and in all probability birth rates will continue to fall, and to fall more rapidly in the rural than in other segments of the population. But, if there is a tendency for

the birth rates to remain relatively higher in noncommercial farming areas then in commercialized and rationalized farming areas, then any element in culture which now or in the future tends to slacken the rapid pace in the direction of rationalization and commercialization will tend to slacken the relatively rapid decline in birth rates.

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The discussion of this topic started with the assumption that population trends, whether in terms of vital statistics or migrations, are correlates of, if not inherent elements in, types of culture. In order to reduce the analysis or speculation to the American farm scene, two extreme types of culture were selected, one the noncommercial, the other the commercial. Even these extreme types are by no means completely independent of each other in our modern society. Channels flow from each to the other by way of a constant interchange of populations and through the channels of transportation and communication. Furthermore, each is overlaid and inter-penetrated, though one much more than the other, by elements of culture from the "great society." The process of acculturation will undoubtedly become more, rather than less, operative in the future, and thus these two extreme segments of rural and urban society will gradually approach each other in types of behavior. To the extent that this is true, urban and rural birth rates and the birth rates in the different rural areas will gradually approximate each other.

Farming, however, is and probably will forever remain different from other economic enterprises. It is not easily subjected to an elaborate division of labor and is quite thoroughly controlled by physiographic and climatic factors, and can probably best be operated by the careful guidance of an owner-operating entrepreneur, whose chief source of labor supply is his own family. Furthermore, because it is a producer of raw products, many of which are easily converted into consumable goods, it retains within itself a capacity to be highly self-sufficient.

It is the very essence of a culture that factors of physical conditioning, uses of technology, economic practices, and ideologies all hang together in a living, working combination. It is out of these living combinations or cultures that population trends arise. If, therefore, rural life continues to be to any measurable extent different from urban life in America, and if certain regions or segments of the farm population live, work, and think differently from other regions or segments of the population, these differences will be reflected in vital statistics. I am inclined to go further and say that our rural development in America has been so unique, if not abnormal, that we may hardly expect trends which arose out of our past to give us sure cues as to the direction in which these trends may travel in the future. What we may expect is that population trends will be integral parts of the trends in cultural patterns. If the patterns are in the direction of noncommercial agriculture, farm birth rates will fall relatively slowly; if they are in the direction of an ever-increasing commercialization of agriculture, they will probably fall faster.

SOME REFLECTIONS OF AN ANTHROPOLOGIST ON THE FUTURE OF OUR POPULATION

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(Read November 18, 1938, in Symposium on Population Growth)

SOME years ago I undertook, in a frivolous moment, to predict what man would look like 500,000 years hence. Despite the impersonal interest, which I anticipated my readers would take in a future so remote, the response to my phylogenetic vision startled me into a recognition of the responsibility resting upon the seer. Today, since I am expected to deal with a more immediate future, I shall be far more modest in my claims.

It is a fundamental trait, shared alike by the individual sorely perplexed by personal cares and by students of population oppressed by statistics, to be concerned with the course of future events. But whereas the desperate individual consults the soothsayer, we seek the advice of science for our population problems; which is wiser, soothsayer or science, is often a moot question. But science, with all its inadequacies, is the only guide we have. Fortunately it is a guide that improves the accuracy of its prediction with each advance in its technique.

The population problems now besetting mankind are profoundly disturbing both to our present comfort and to our future prospects. Quantitative changes of such character are taking place that the need is urgent for an appraisal of our prospects. To those who foresee future doom and deterioration growing out of these changes it is imperative to assess the nature of present trends and to predict their future courses with a view to forestalling the evils adumbrated. But, even to those who study popu-

lation problems with a calmer eye, the necessity is apparent for knowing where current population adjustments are likely to lead us and what measures might be effective in improving the quality of our population. For no one, I trust, regards the present status of mankind as the ultimate level of human perfection.

I have assumed that such reasons as these have stimulated the organization of this symposium, and that fields of investigation have been selected for discussion which seem likely to shed some light on the problems with which we are all concerned. My task in this session is the appraisal of the anthropological prospects of population and since that covers a wider pasture than I could possibly cover in half an hour without causing you to suffer from an unpleasant attack of mental indigestion, I am taking the liberty of discussing only certain generalities which seem to me to be significant. Moreover, since the quantitative aspects of population have, in the past, received the major share of attention, I shall confine myself to a discussion of the ways in which population quality may be modified. I shall, however, in the limited state of our present knowledge, attempt to indicate probabilities rather than eventualities, and I shall lift the monitory more frequently than the minatory finger.

Although biological evolution has occupied more scientific attention than any other concept, it has hardly entered into the discussions of students of population. The reason is not far to seek. Most students of population have been concerned with the behavior of past or present population, and when their findings have aroused concern for the future, this concern has been limited to the immediate future. The more distant epochs to come appear far too uncertain to evoke more than passing interest. Evolution, on the other hand, represents a slow, often extremely deliberate process, the effects of which are rarely discernible within the limited span with which populationists wisely permit themselves to deal. From the biological evidence at our

disposal human evolution within the historic period has been negligible. Whether we face a period of quickened evolutionary activity is unpredictable, but it seems safe to say that no evolutionary trends are evident of a nature to cause concern for the next few centuries at least.

In a few biological traits, however, some students profess to detect evidence of a rather rapid evolutionary movement. Human dentition appears to be growing progressively worse. Surveys of school children disclose an appallingly high rate of caries and dental abnormalities. Whether these defects are the expressions of an inevitable evolutionary adjustment or represent the consequences of modern diets, which may be checked by dental science, is uncertain.

General bodily size, also, has undergone in the last century a phenomenal increase with each successive generation. The trend has been definitely detected in a large number of groups. In Europe the recruits to the ranks of various national armies become taller with each decade. In this country the same tendency has been revealed in our army and university records. Recently, Japanese students have reported a similar change in their population. I believe that we may expect a continued increase in the size of our citizens in the next few generations, but I doubt that this tendency will continue uninterruptedly. It appears to me to be a biological response to various environmental factors, rather than an evolutionary trend, and should become stabilized with the attainment of optimum environmental conditions. From the qualitative point of view, there is no evidence that this increment in size is an advantage. Increase in size is often used as an index of biological superiority, and it is and has been frequently found associated with increased intelligence quotients, but one may seriously question the biological efficiency of greater over lesser bulk, and the association of superior intelligence with greater size may well be a fortuitous circumstance rather than cause and effect.

Evolution, therefore, presents no imminent danger to our population, nor does it seem likely to modify appreciably the quality of the human organism in the near future.

Another possible source of qualitative change exists in the variations of milieu. The importance of environment as a factor essential to the development of human traits is an obvious truism. In one sense every organism, more or less stabilized by heredity, is an expression of its environment. Unfortunately, for the progress of our insight into human variation, environment and heredity are frequently sundered and given false emphasis. We are just emerging from a period in which the rôle of heredity, enjoyed far more biological favor than environment. Studies are now accumulating, however, to demonstrate that within fairly wide limits human characteristics may be altered by variations of milieu. Children bred under favorable conditions, with adequate diets, reveal considerable increments in size and in intellectual performance over those raised under inferior circumstances. Boas' studies of growth demonstrate the effect of an unfavorable milieu in disturbing the growth tempo which, other things being equal, tends to be the same in all groups. My own investigations on the Japanese born in Hawaii reveal an astonishing increase in size, but as I mentioned earlier, size alone is not a critical index of quality. The conviction that psychological performance also responds to variations in environment is becoming more general as controlled investigations continue to confirm this view.

We may consequently predict that as our environmental status shifts so will the average quality of our future populations. If the degradation now characteristic of part of our country persists and even spreads, we may, indeed, view the prospects with alarm.

The changes in the quality of our population which may be produced by race mixture is a problem of great significance, but one with pitiably little precise documentation. In this country race mixture as a population factor looms

far higher on the horizon than almost anywhere else in the world. In Europe and in Asia the populations are relatively stabilized around old mixtures, and, unless unforeseen movements occur there, the successive generations will tend to perpetuate their present integration. Here, on the contrary, vast numbers of the diverse populations of Europe have been thrown into an intimate contact which they never knew in the old country. To add to the confusion, we have also received large contingents from Africa and lesser ones from Asia.

Such a conglomeration of various races, cultures and traditions has, of course, given birth to one of the greatest of our social problems. Through historical and economic causes operating both in this country and in the Old World, the stream of varied migration which deposited millions of our people on our shores became too torrential, and instead of a homogeneous population we have witness in our social structure and political machinery of large unassimilated masses of people, subject to discrimination and prey to emotions engendered by their minority position. Such a state of affairs represents a vicious circle in which a deepening group consciousness evokes an increasing discrimination which in turn intensifies the group consciousness. In the earlier days of our history, prejudice was by no means absent, but it failed to check the natural miscegenation which the simpler conditions of that society encouraged. Certain groups, such as the Pennsylvania Dutch, were able through geographical circumstances, to maintain their cohesion, but in the more cosmopolitan seaboard, and in the reshuffling process of the frontier, French, Dutch, Swedish, English, Scotch, and Germans mingled rapidly, but as the flood of migration increased, groups of foreign origin arrived in numbers too large to be absorbed by this type of assimilation. Today, as a consequence, the populations of our industrial and mining centers reveal numerous lines of cleavage that follow national origin. Up to now these lines have tended to slow

down the process of miscegenation, but in the future we may expect an increasing amount of group mixture as the successive generations become more intimately adjusted to American life and move into other areas. What the effect of such miscegenation may be no one knows with any certainty. Some students, touched by the teaching of Gobineau and Huston Stewart Chamberlain, see disaster following on the trail of an extensive mingling of the older stocks of Northwest European origin with the newer arrivals from Southern and Eastern Europe. Others anticipate a burst of biological vigor, which frequently accompanies hybridization. The essential point here, however, is whether or not biological qualities are correlated with race. The prevailing opinion among anthropologists denies the existence of any significant correlation of quality and race in the population of Europe. The evidence which exists favors the belief that the races of Europe are so closely related and so mixed, and overlap each other to such a degree that race is inadequate for the determination of qualitative distinction. Of far greater value for prediction purposes are the qualities contained in family lines.

Less unanimity, however, exists with regard to races of more diverse origin. Even after allowances are made for unfavorable environment, the negroes of this country appear to be more susceptible to certain diseases than are the whites. On the contrary, they are less prone to others which are more virulent among whites. The psychological testing which has been carried on among negroes is, I believe, taken to be inconclusive and has been criticized by many psychologists. It is apparently true that the performance of negroes on psychological tests improves under more favorable environmental conditions. But whether or not one takes the stand that negroes are differentiated from whites in certain qualities, one need have no misgivings for the immediate future. The social prejudice which exists throughout the country erects an effective bar to any widespread miscegenation. Inter-racial mixture does,

of course, exist, and although its extent is anybody's guess, the offspring of such unions usually find themselves prevented from complete assimilation. There is, however, a steady trickle of very light mulattoes who "cross the line." I have heard various estimates given of the size of this movement, but it is probably not more than several thousands annually. How much effect this movement might exercise, even if we assume a qualitative difference among negroes, is problematic. The chances are that it would be negligible, both on account of the small numbers involved and because hybrids light enough to pass are biologically far from being negro. It seems to me that for some time to come we shall continue to have about 10 per cent of our population, popularly classified as negro. For the movement of the negroes into the crowded industrial centers merely serves to segregate them more effectively than before.

The Asiatic contingents are relatively so unimportant that they need not occupy us at this time.

The currents of population movements which have been set up in this country, as a consequence of economic developments, represent a significant social trend and one which might also conceivably have repercussions on the prospects of our future population. In the past the principal movement was a uniformly westward one. At present the major trend appears to consist of a great many local eddies, carrying population from the farm to the local urban centers. These local movements in turn feed a current leading to the larger industrial centers of the United States. The older eastern areas have suffered both these processes of population erosion and deposition. New England, for example, has been drained of its youth ever since the late 18th and early 19th century in two streams; one attracted to the richer farming areas to the West, the other seeking advancement in the large cities of the Eastern seaboard. At the same time this area has, at its industrial centers, attracted large numbers of French Canadians,

Italians, and Poles who have undertaken the jobs discarded or avoided by the older stocks. Although these national population movements are often selective they tend to keep the population as a whole in a constant state of readjustment. Consequently, they serve to break up the unassimilated groups formed by a too rapid flow of immigration and to increase the opportunities for miscegenation. Present population movements, therefore, are likely to produce a more homogeneons nation in which the various racial elements are more evenly welded. From this point of view the process can be regarded as a wholesome one. The qualitative consequences, as I pointed out previously, are in considerable uncertainty, since qualitative differentiation between the various national groups composing our population cannot be unequivocably established.

Let us now pass on to the effect of differential birth rates upon our future population. The disparities which at present exist between the various occupational, economic, social, and geographical groups in this country are too well known to this audience to require any emphasis or reiteration. Such differentials of birth rate and death rate might conceivably have far greater consequences in replacements of various elements in the population than any other factor. They are consequently dynamic aspects of population which should concern us most deeply, but before we become impelled to take steps to alter the dynamics of these differentials, it seems to me that we must first determine whether or not superior quality is highly correlated with superior economic or occupational position and whether the corollary is equally true. Actually what do we know about such qualitative associations? We know these facts: (1) The children of superior economic groups are larger, tend to mature earlier, and are somewhat less susceptible to disease. (2) The children of superior groups take higher scores on intelligence tests than do those of inferior economic position.

Now, as I have previously emphasized, increase in size

has not been shown to be inherently a biological advantage. We may admire it, but esthetics and biological efficiency do not necessarily go hand in hand. Moreover, size is apparently a character which is partly dependent on milieu and may be significantly increased or diminished by favorable or unfavorable environmental conditions. In other words, the present inferior size of the lower economic groups may not be an inherent trait at all. The same applies with equal force to the onset of maturity and susceptibility to disease.

The association, however, of higher I.Q. ratings with superior economic position appears to have rather more substance. Although here, too, part at least of this superiority may be attributed to advantageous circumstances. Many believe that the selective process has in general elevated the more vigorous and capable families to the more favorable situations in society.

In previous periods of our history, it is contended, the superior groups kept pace with or even exceeded the increase among the inferior groups, whereas now the superior strata of our society are failing to reproduce themselves. According to this view such a disequilibrium can only lead to a steady depletion of the most desirable strains in our population. Although, as I have already suggested, the evidence of a high degree of correlation between quality and socio-economic position is not unequivocal, yet as a matter of common sense one might expect to find in a society such as ours a freely selective tendency leading to a general stratification according to ability. The danger, however, lies in overweighting this natural tendency for the most able to rise and in underestimating the currents of cross-fertilization within a population that make for the wide distribution of genetic potentiality. In a population with balanced reproductive rates, the anonymous mass of the people are endowed with the regenerative power to replace its peculiar genius and to maintain its characteristic level.

France, for example, has through a long and glorious record replenished her great from among the peasantry as well as from the aristocracy. Throughout the history of Europe, selection has been operating to elevate certain houses into the aristocracy. These families in time become extinct and are replaced by newer candidates from lowlier stations in society. The turn-over in the course of a millennium has been considerable, with no apparent deterioration. Someone has observed the amazing paucity of really ancient families in Britain where aristocracy is an ancient institution, offering a maximum protection to antiquity. Apparently the general distribution of genes in a population makes it probable that new families of ability will arise from the commonalty to replace those which become extinct or slip back into mediocrity.

I, therefore, entertain a greater optimism than many students in the recuperative powers of our general population where reproduction is balanced, but I do admit a certain degree of uneasiness over the atypical reproductive rates characteristic of the various levels of our society which obtain at present. Such a disparity is deplorable more because it affects the anonymous mass than because it tends to eliminate the topmost levels. Some comfort, however, may be derived from the ever increasing adoption of birth restriction throughout the population. In fact, the spread of contraceptive practices seems to me to offer a practical measure by which to halt the present disequilibrium.

Finally, I wish to discuss briefly the dysgenic factors inherent in our present medical and humanitarian practices. It is apparent that as medicine becomes more effective, more of the inferior progeny of our population will be saved from extinction and will be nurtured to maturity by our humanitarian instincts. We may expect such inferior organisms to reproduce similar ones. We thus interfere with the process of natural selection, create an increasing burden on society, and even threaten the soundness of our

stock by permitting inferior strains to multiply and spread. This is the picture that more and more observers of contemporary society paint. The extent of the damage and the rapidity of the resulting decay is estimated differently by various writers. The truth of the matter is that no one knows. Unquestionably we cannot deny the theoretical danger inherent in humanitarianism, but before we steel our hearts against the weak and the helpless let us be sure that they are indeed the weak and the helpless.

I think it is evident from my preceding comments that our most pressing deficiency in the study of population and in the assessment of its prospects arises from our ignorance of the qualitative aspects of the various components of our society.

Whether correctly or not, many students have "viewed with alarm" the imminent disasters which they believe threaten contemporary populations as a consequence of various trends found to be operating within these human aggregates. To a large extent these trends are quantitative movements determined by statistical methods and easily checked and verified. The disasters they portend, however, consist of qualitative changes that are alleged to be correlated with the quantitative adjustments. These qualitative correlates, on the contrary, are difficult to substantiate, partly through lack of data and partly because the techniques for determining the quality of human populations are inadequate.

The quantitative study of human populations has already accumulated a considerable history, perfected instruments of research, and amassed a great corpus of exact data. Governments interested in the size of their populations have created permanent census agencies which provide a continuous flow of current information. Parish records, registration offices, social and economic bureaus help to feed the hunger for more exact and refined sources of quantitative information. All this and much more besides have made possible precise knowledge on the growth of our

population within certain time limitations, the rates of increase or decline of various classes within the total aggregate, the birth rates and death rates of the total population as well as of the component units. But essential as these researches are, they are not enough, and the very investigators of the quantitative aspects of population have been among the first to recognize this weakness in their data. The quantitative study of population without concomitant qualitative information is like an elaborate system of bookkeeping without the use of dollars and cents. Such bookkeeping provides information on the production and movement of merchandise, but it fails to cast up a balance of profit and loss until time itself provides the answer in the accumulation of earnings or in the bankruptcy of the business.

Unfortunately, anthropology as yet cannot provide the monetary system, and many believe that the currency of psychology is definitely off the gold standard. This then is the crux of our problem. The student of the quantitative phases of population asks the anthropologists what qualitative meaning their investigations have. And we should reply, even if some of us don't, that quality is something we know very little about.

The field of human population, therefore, presents an unbalanced condition in which the numerical studies far outstrip the investigations of quality. Such a disparity creates a serious danger against which we must constantly guard. Students of population are under overwhelming pressure to interpret their quantitative results, and in the absence of qualitative facts, firmly established by scientific methods, they are exposed to the insidious temptation to substitute for them subjective judgments of a qualitative nature.

This lamentable state, I am, unfortunately, unable to ameliorate. I hope, however, that this discussion may have the effect of emphasizing the most pressing need in the study of human populations and of clarifying the issues

to be solved. Futility is a harsh word, but it seems to me that unless we turn our attention to the qualitative meaning of population movements our investigations will become increasingly futile and sterile; numbers alone have little meaning.

I have in this talk continually stressed the inadequacy of our knowledge for prediction purposes. Although caution makes a poor steed for prophecy, I should like nevertheless to conclude with a few mild capers of prediction.

I predict that our population is now entering a period of stabilization during which it will become increasingly homogeneous. The immigration of foreign elements has slowed down and the various national groups have ceased to receive recruits from abroad. The cleavages which now exist will tend to disappear as the various minority groups come more and more under the influence of local adjustments and national movements. Internal migrations, economic improvement, increased education, and countless other social agencies will continue to shuffle our population thereby exposing it to varied contacts. This process leads to miscegenation and increasing homogeneity. The cores of these minority groups will, of course, continue to exist for a long time, but the tempo of mixture once under way will advance rapidly. The resulting mixture will not necessarily represent an inferior population. There is every reason to expect a heightened vigor and a wider range of ability as a result of hybridization. The negro population will, however, continue as a distinct group with only minor seepage into the major mass of the population.

The population in the near future will exhibit some qualitative improvement as the result of continued amelioration in the living conditions of a large part of our country. This will raise the average level, but not particularly affect the superior levels of ability.

The present destructive tendencies inherent in the differential birth rates of the various groups in our population and in the dysgenic effect of increased humanitarian-

ism may, if unchecked, tend to offset the gains anticipated from mixture and from improved environment. But I predict a more universal application of birth restriction, with balanced birth rates more like those of the pre-contraceptive period.

The dysgenic consequences of medicine and humanitarianism will probably increase for some time to come. Not until education has enlightened the mass of the population will steps be taken to reconcile the health of the nation with the health of the individual.

CONTACT POINTS OF POPULATION STUDY WITH RELATED BRANCHES OF SCIENCE

ALFRED J. LOTKA

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(Evening Lecture, November 18, 1938, in Symposium on Population Growth)

President Morris:

Before I formally start tonight's program I wish to address myself expressly to you and to the Officers and Members of the American Philosophical Society. It is recorded that a certain young man with literary aspirations, on meeting a famous author, sought his advice. What is the essential in acquiring excellence of style? "My friend," the answer came, "the first requirement is to have something to say that will not stay unsaid." President Morris, I have that to say to you which will not stay unsaid. I want to tell you on behalf of the members of the Population Association of America and on my own, how deeply we have appreciated the splendid hospitality extended to us on this occasion. For myself I can say, and I know my associates will join me, that I shall long remember this meeting as one of the most auspicious within my experience of scientific gatherings.

And now I turn to address myself generally to the members of the two societies here assembled.

When it was suggested that I should address you on this occasion, and I reflected on a topic which I might choose for your entertainment and profit, I found myself confronted with a problem—not a problem of fact, but a problem of grammar. "Contact Points of Population Study with Other Sciences" was my first thought. But then I asked myself: Is Population Study a Science? A delicate

question which I should prefer to avoid. So then I compromised: Contact Points of Population Study with Related Branches of Science, on the principle that one may be related to a king without being a king.

But the title is still a dangerous one. For, in words borrowed from Longfellow, it suggests endless toil and endeavor, and tonight some of us long for rest. But let me reassure you. I propose only to sip from the cup of wisdom, not to drain it.

All branches of science are necessarily concerned with observations or events located in space and time; but there is a characteristic variation in emphasis as to the role which time plays in those observations and in the organized knowledge founded on them. At one extreme we have such a science as dynamics, in which we are concerned with relations involving only lapse of time, $t_2 - t_1$, regardless of the particular date of the observations.

At the other extreme we have such sciences, historical sciences, as geology, in which essential interest attaches to particular epochs or dates when specific events took place or specific situations existed.

It is to this latter class of sciences that population study is most closely related. We are interested primarily in concrete situations, actual populations in the past, present and future.

Our most intimate link with the near past, in population study, is genealogy. When we consider what a simple thing it is, for any but the illiterate, to keep adequate family chronicles, it is remarkable how scant are good genealogical records. I know of no statistics on the subject, and therefore I can only raise the question: I wonder what proportion of the population at large can give an account of its ancestry with any kind of completeness six or more generations back? However that may be, we may expect that future students of genetics will be better supplied with data of this kind than we have been in the past. This is certainly to be desired, for in a sense the period of a gen-

eration may be said to be the natural time unit in the biology of a population; evidently the opportunities for organic evolution to operate are proportional to the number of elapsed generations.

This thought is worth dwelling on. We are ordinarily rather impressed, not to say crushed, by the immensity of time required for the process of organic evolution. But let us engage in a little exercise in constructive imagination. I say constructive, because I am going to ask you to summon before your mind's eye, not fictitious characters, but real persons.

I am inviting you to a stag party in the Franklin Field Stadium. It is filled to capacity, seating 85,000 persons, men. Each of you has the privilege of inviting his own guests to this gathering, but for each of you it is a distinct and particular party, a stag party, as I have said, except for your presence, if you are of the fair sex. For, these men assembled in the Stadium are your male ancestors in direct line of descent. Quite near, on your right, sits your great-great grandfather, and his father, and grandfather, and so on. By the time you have run your eye around the first row, you have reached the 1,000th generation, your ancestors of 20,000 or 30,000 years ago. In fact, if the seats extend all the way around the stadium, he is sitting next to you on your left. I hope he does not incommode you. But there is a second row, and a third, and so forth, to the 85,000th ancestor in direct line. His date is difficult to gauge, for the generations of his day were perhaps rather short. But it is pretty sure that the calendar would record the year of his birth as a seven figure number B.C. And to look at that creature, up there in the top row, you would hesitate to say that those were the days when *Men Were Men.*

But let me again remind you that this is not a party of hypothetical beings, or make-believe phantoms. These figures sitting in the stadium (in your mind's eye) are true objects, each and everyone as definite, as real, as you and I.

So viewed, the links in the chain that tie us to the past appear almost too intimate; figured in generations the eons shrink, and the dawn of time seems less remote.

But the uncertain focus of the mind's eye does not satisfy the scientist. Ethnologist, archæologist, anthropologist, palæontologist, cosmologist—each contributes in the endeavor, by concrete evidence, to lend sharper outlines to our conception of the proximate and ultimate origin of our human population.

Now the generation, though in many respects the appropriate and indeed the natural unit in biological time scale, has the defect of being a rather elastic unit, if I may say so without contradiction of terms. Let me illustrate this. On any one day here in the United States, there are born something over 5,000 babies, of which about one half are girls. Let us fix our attention on such a one day's crop, born say at the zero hour, or to simplify matters, let us consider 1,000 newborn girl babies (the "zero" generation) and follow up their progeny throughout life. Their daughters—the first generation we will call them—will be born anywhere from 10 to 55 years after the zero hour, or let us say between time $t = 10$ and $t = 55$. Their granddaughters will be born between $t = 20$ and $t = 110$; their great-granddaughters between $t = 30$ and $t = 165$, and so forth (see Fig. 1). There is thus an ever increasing overlapping of generations, with some rather singular results. At time $t = 200$ there might be coexisting persons from 16 different generations of descendants of our zero hour crop. Actually, because of the great thinning out of the curve of distribution of births at the two ends, only about four generations are likely to be represented. Even this is noteworthy. If you could trace your full family tree back say to the sixth ancestor in direct line, you would find that the birth dates of your relatives of your own generation might well be scattered over a period of 70 years or more. That is, throughout your whole lifetime, off and on, you might be receiving announcements of the birth of

what I might call your sixth generation cousins, if their parents took the trouble to inform you. Another aspect of this matter is that, sitting right next to you here in this hall, may be a person who is five, six or more generations your senior, so to speak, or your junior. And, since a generation from mother to daughter measures on the average 28 years, from father to son 32 years, there arises a sort of dislocation of generations of the two sexes. In the course of 224 years, there is room for eight of your female ancestors in direct line, but only for seven of your male ancestors in direct line, if we reckon in terms of average

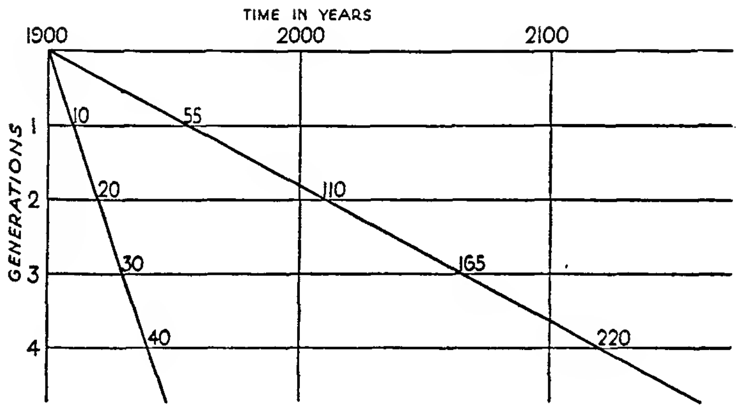

FIG. 1.

figures. This may appear rather puzzling, since each of your male ancestors was married to one of your female ancestors, and there is at first sight a difficulty in matching eight males with seven females. The answer to this puzzle can best be explained by a diagram. In Fig. 2, we see on the left a string of seven successive male ancestors, which for the sake of our illustration are spaced exactly 32 years apart. On the right is a corresponding string of eight female ancestors, spaced 28 years apart. Now, in general, each of these persons will have married some mate suitably indicated on the diagram, with whom we shall not

further concern ourselves. Let us consider the case in which the last two of the chain are a married couple and the first two of the chain are a married couple. On tracing the successions in this diagram it is seen that under these

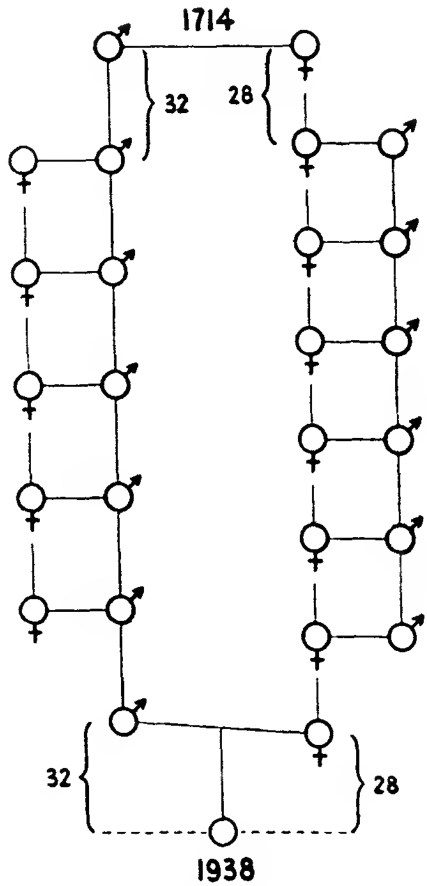

FIG. 2. Seven male and eight female generations in 224 years.

conditions number one in the male chain is male ancestor of *seventh* order through the male line of descent to the child born of the last couple, while he is ancestor of *eighth* order to the same child through being married to female number one.

Somewhere along the line one of your forebears may have played a duplicate part. It is as if your great-grandfather were at the same time your great-great-grandfather, except that this remarkable double act of one person in two ancestral roles is somewhat farther removed than merely the third or fourth generation. If my diagram happens to apply to you, it means that your great-grandfather seven times removed in direct male line of descent, was at the same time your great-grandfather eight times removed, by virtue of the fact that he was married to your great-grandmother eight times removed.

I might go into further detail with a pictorial representation of some of the pertinent relations between successive generations, as spread out in time. But it will save steps if I pass on immediately to another matter, in which similar principles will be illustrated.

Modern man, as pointed out years ago by Emerson, "is not contained in his skin." The unit of industrial society is not one person, but one person plus a quota of accessories. These accessories have a "life period in use" of their own. This brings us in contact with two classes of problems in economics: in particular, for those accessories that have a life period shorter than human life, there is the problem of periodic replacement. For those accessories the life period of which exceeds the human life span, the problem of inheritance and of the resulting distribution and redistribution of personal property arises. The latter problem I do not propose to discuss here. The former has a peculiar interest for us as students of population, because of the close analogy which it bears to certain problems of population growth. Suppose, for example, that a fleet of 1,000 motor trucks is put into operation at the beginning of a certain period which we shall call time zero. As time goes on some of the trucks become disused through accident or wear. Suppose each such truck is immediately replaced by a new one. If the equipment is to be kept constant, what will be the replacement rate as time goes on?

The problem is identical in form to one considered a good many years ago (1909) by the French actuary Herbelot, namely the replacement rate in a body of insured policyholders which is to be kept constant in number as the ranks of the charter members become thinned by deaths. The nature of the problem, and the type of analysis by which it is solved, is illustrated in Fig. 3. The hat-shaped curves in this drawing show the distribution in time of "replacements" in successive generations of disused articles, computed on the basis of observations compiled by E. B. Kurtz in his *Life Expectancy of Physical Property* (Ronald Press, 1930).

Without going into details of mathematical analysis, I may briefly remark that the relation between the distribution curves of successive generations takes the simple form that the nth semivariant of the jth generation is equal to the jth multiple of the nth semivariant of the first generation.[1]

We have thus noted one contact point of population study with economics. We shall note others as we proceed. The mere passing reference to life insurance at once turns our thoughts to the economic relation of the wage-earner to those who share in the benefit of his earnings. Through this circumstance, and quite apart from any sentiment, he becomes an economic asset to his family. It is a relatively simple and straightforward matter to compute his capital value to his family from this point of view, if we base our calculations on average mortality and on a suitably selected scale of earnings and expenditures. As this subject has been fully set forth in a volume[2] published a few years ago, by Dr. Dublin and myself, I shall

[1] For details see A. J. Lotka. "The Progeny of a Population Element," *American Jl. Hygiene*, 1928, **8**, p. 875; "The Spread of Generations," *Human Biology*, 1929, **1**, p. 305; "Industrial Replacement," *Skandinavisk Aktuarietidskrift*, 1933, p. 51; "The Application of Mathematical Analysis to Self-Renewing Aggregates, with Special Reference to Industrial Replacement," to appear in the issue of March 1939, of the *Annals of Mathematical Statistics*.

[2] L. I. Dublin and A. J. Lotka. *The Money Value of a Man*, Ronald Press, 1930.

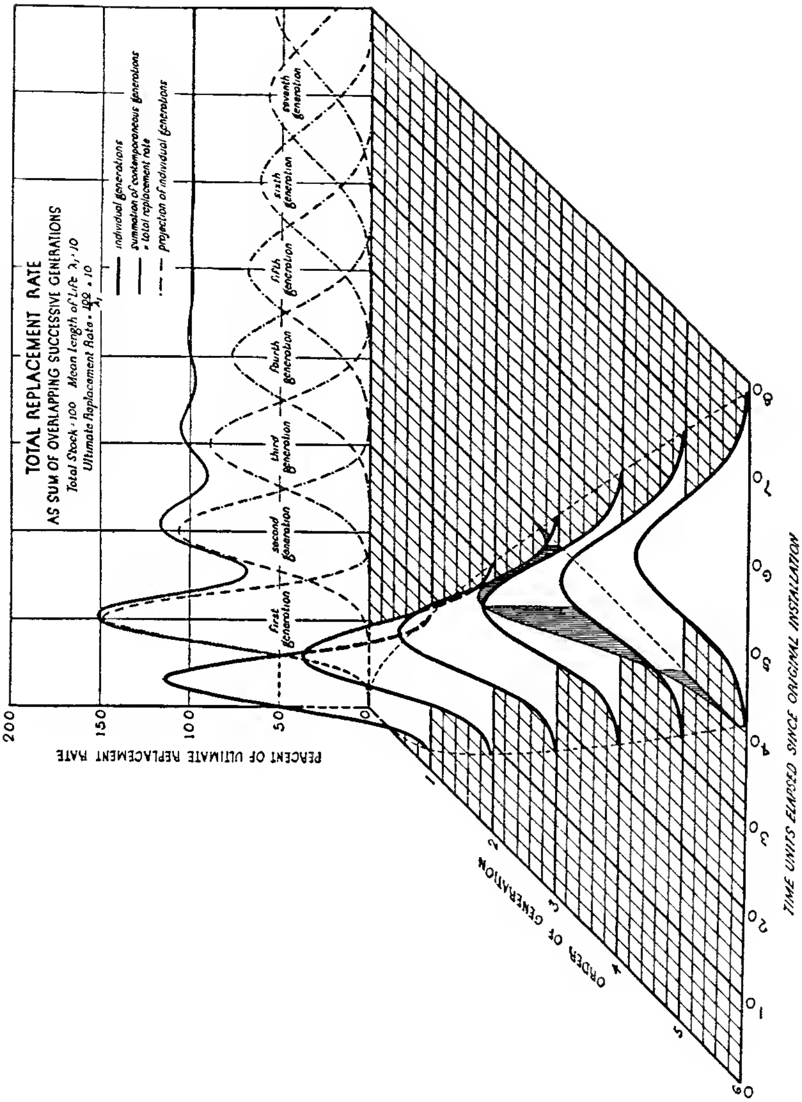
TOTAL REPLACEMENT RATE
AS SUM OF OVERLAPPING SUCCESSIVE GENERATIONS
Total Stock: 100 Mean length of Life $\lambda_1 = 10$
Ultimate Replacement Rate = $\frac{100}{\lambda_1} = 10$
individual generations
summation of contemporaneous generations = total replacement rate
projection of individual generations
first generation
second generation
third generation
fourth generation
fifth generation
sixth generation
seventh generation
PERCENT OF ULTIMATE REPLACEMENT RATE
ORDER OF GENERATION
TIME UNITS ELAPSED SINCE ORIGINAL INSTALLATION

FIG. 3.

not go further into the subject here. There is, however, one angle of the problem which calls for mention. Not infrequently, the question of the value of a man, not to his family in particular, but to the community as a whole, has been mooted. Now, this is a very different thing from the value of a man to his family. This is sufficiently clear in extreme cases, where a man by unethical and anti-social means secures for himself and his family large profits to the detriment of the community. But the difficulty arises not only in these extreme, and, let us hope, rare cases, but lies deeper than this.

The general sense of well being in a community, other things equal, rises and falls with the proportion of goods available per head, that is, with the quotient

$$\frac{\text{economic goods}}{\text{number of persons in the population}}.$$

Now this quotient is increased if the numerator increases while the denominator is constant. But it is decreased if the denominator increases while the numerator is constant. This shows at once that quite generally we can not discuss the value of an individual to the community in the same way as we consider the value of ordinary economic goods. It must be remembered that the individual is both a producer and a consumer. Whether the addition of one more individual to the community is an asset or a liability depends on whether his contribution by way of production exceeds or falls short of his consumption. While this brief statement is not put forward as fully covering the issue, it certainly should serve to point very clearly to the fact that the problem of estimating the value of an individual to the community is something very different from that of estimating his value to his immediate dependents and would, if it can be solved at all, demand entirely different methods.[1]

But these are rather special applications, to which we have been led primarily by formal resemblances between

the growth of the living population and that of his inorganic accessories, rather than by similarity in pertinent fundamental principles. I shall now ask you to take with me a broader view of our topic, in the course of which we shall again find ourselves contacting with economics.

When our Association was founded, we duly weighed whether it should restrict itself to the study of human populations, or whether it should include also that of populations of living organisms generally. It was decided that the prime object of study, at least, should be human populations. But I believe that on such an occasion as this it is appropriate for the nonce to take a broader view and give some thought to the wider topic, the subject that may be denoted by the term *General Demography.* I feel that this is the more justified since the human population is dependent on coexisting animal and plant species, for articles or materials of food, clothing, shelter and so forth. At the same time, certain other species figure in our scheme as nuisances, either by engaging with us in honest competition for food, or, with less respectability, by stealing from our granaries what we have sown and grown. Still others threaten us by direct attack on life—rarely, in modern civilized communities by honest frontal attack; all too commonly by sneaking invasion under cover of microscopic and ultramicroscopic dimensions.

The descriptive study of the interrelations between coexisting species and, more generally, their environment, is the province of *Ecology.* Since in Nature the devouring of one species by another plays so large a role, a not inconsiderable part of the ecologist's studies are devoted to *food chains,* and networks, of intermeshing food chains. C. Elton has pointed out the interesting fact that such food chains will not usually exceed five successive links, since ordinarily a predatory animal must be of overpowering dimensions as compared with its prey.

To this latter rule man makes a notable exception, at least if we estimate his strength by his bodily energy alone.

In his dietary he includes whatever suits his palate, regardless of size, from whale to sardine. The practical problem of his food supply is the concern, on the one hand, of the agricultural economist, on the other of the economic ecologist. On the latter devolves, among other things, the task of taking a census of animal and plant populations in the wild state. Various methods have been evolved for the investigation of this and related problems. A novel method, that might be applicable to certain special cases occurred to me recently, and if you will promise not to take it too seriously, I will divulge it to you.

While taking a carriage drive during a recent stay in Bermuda, I was pleasantly entertained by the melodious piping of the population of tree frogs. The thought occurred to me that by means of modern noise recording instruments it might be possible to obtain a measure of the density of frog population. This is the experimental side of the problem. Analytically, the rather pretty problem arises of determining the intensity of sound at a particular point, in a field studded with individual sources of a given intensity and a given distribution. The idea appeals to our sense of humor. Let us give this scheme the benefit of the doubt whether perhaps it may have some serious interest.

This logically leads me to speak of the mathematical analysis of ecological problems, more particularly of the interrelation in the numbers of organisms of different species inhabiting a given locality. This analysis is a very recent development, to which, in fact, so far a mere handful of authors[1] have contributed. The first systematic development of the subject dates from 1925, though preceded by a number of scattered publications dating back to the early years of the current century. Notable contributions have come from V. Volterra since 1927. He had been led into this field by a practical problem brought to

[1] This development originated independently in America (Lotka, 1907, 1925), Italy (Volterra, 1927), and Australia (Bailey and Nicholson, 1931).

his attention by Professor U. D'Ancona, in connection with Italian fisheries. The relation of this general subject to sea fisheries had already been noted by me in my book *Physical Biology,* 1925. Experimental work conducted with these analytical studies as background has been published quite recently by G. F. Gause.

But the interrelations of species of living organisms with their environment extends beyond the limits of their own substance. The ancient who reminded us: "Dust thou art and unto dust shalt thou return" may have somewhat heroically simplified his chemistry, but he had hold of a fact of deep fundamental significance. Modern geochemistry and cosmology, by throwing the picture into sharper focus, have served only to emphasize its meaning. For we know now that it is strictly and literally true that we are pieces of earth—selectively sublimated pieces, it is true, of peculiar composition and still more peculiar mechanical properties—but nevertheless, pieces of earth, moving about on the earth.

To our near view, as spectators who mingle freely with the action on the stage, certain of the individual steps in the process by which we acquire this strange mobility are so prominently displayed, that we lose sight of the basic phenomenon, and have to be reminded of it. As Keyser has remarked: "Man has long been wont to regard himself as a being quite apart and not as part of the cosmos round about him. From this he has detached himself in thought, he has estranged and objectified the world, and lost the sense that he is of it. And this age-long habit and point of view, which has fashioned his life and controlled his thought, lending its characteristic mark and color to his whole philosophy and art and learning, is still maintained, partly because of its convenience, no doubt, and partly by force of inertia and sheer conservatism, in the very teeth of the strongest probabilities of biological science. Probably no other single hypothesis has less to rec-

ommend it, and yet no other so completely dominates the human mind.''

As to the ''strongest probabilities of biological science'' to which Keyser here refers, the words of Vernadsky are apposite: ''L'organisme réel est indissolublement lié au milieu ambiant, et nous ne pouvons l'isoler que dans notre pensée.''

It would be interesting, if time permitted, to quote pertinent passages from many writers. What is particularly significant is that scientific investigators and literary men, approaching the facts from widely different angles, arrive at the same point of view.

It is a pity that ordinarily we fail to entertain a vivid realization of our unity with nature, for its full experience affords a deep esthetic satisfaction, rising in some persons and on some occasions to the heights of ecstasy.

But these, for most of us, are but transitory moods, and the spade work of science has to be done in the calmer light of controlled reason. To our critical faculties the world of living organisms appears as a special stratum of the earth, the biosphere, extremely thin, as geological strata go,[1] but abundantly making up for this by its extraordinary activity, which is on a planetary scale. Some indication of this is given by the quantities of substance taking part in the organic cycle. The respiration of animals pours into the atmosphere annually something of the order of 3 or 4 billion tons[2] of carbon dioxide, of which about one half billion is the contribution of the human species. But man alone, by the combustion of coal and other fuel in home and in industry, throws in another 3 billion tons. Altogether, these discharges amount each year to a very notable fraction, perhaps about 1/300th, of the total carbon dioxide contained in the atmosphere, so that we are definitely in

[1] It has been estimated that if the whole of living matter were spread uniformly over the earth, it would make a film about one half inch thick.

[2] The figures quoted in this paragraph are necessarily rough approximations intended only to indicate the order of magnitude involved.

the presence of operations on a planetary scale. A part of the carbon dioxide discharged[1] is again reduced and assimilated by plants under the influence of the sun, which keeps the cycles of this great world engine revolving.

The goings-on in the biosphere are not only on a vast scale; they are immensely complicated. But the complication is in the variety of details. In the fundamentals a systematic pattern is discernible.

What we have before us is a system composed of aggregates (populations) of energy transformers. Each aggregate or species consists of a multitude of essentially similar units built of perishable materials so united into a working structure, that by its operation each unit gathers into itself the substances necessary for its repair and growth, including periodic replacements, and the energy necessary for its operation.

The material for repair and growth each transformer draws from its environment, including, very commonly, individuals of another species. And so there is continually going on an interchange of matter between the various species, the matter of the system as a whole undergoing corresponding changes in distribution among its component species.

But there are limits to this inter-convertibility. We are all familiar with the entertaining spectacle of the patient cow systematically engaged in the profitable and right pleasant pastime of converting grass into beef, and it is largely within the control of our agriculturalists to assure that this sort of thing shall be a continuous performance. But even our greatest animal husbandry expert cannot, today, bring about a similar conversion of all manner of herb into mammoth, though not so very long ago this must have been a daily event.

At this point in our reflections we may profit by the experience gained in another field of science, where also the

[1] Another important source of atmospheric carbon dioxide is volcanic discharge.

study concerns the distribution and redistribution of matter among specified components of a material system, namely physical chemistry, and more particularly chemical dynamics including statics and kinetics. In that case the components in terms of which the state of the system is defined are so-called chemical elements and compounds. The fundamental laws of chemistry, though allowing a legion of possible transformations, nevertheless do limit them by what, analytically, takes the form of reaction equations, or equations of constraint. Now, for physico-chemical systems these equations of constraint are fixed once for all, and the evolution of such a system, at any rate in the simpler cases ordinarily considered, takes place, as it were, by motion forward or backward along a single track line. This is because the multitude of chemical elements and compounds form a manifold of discrete entities, separated from each other by a hiatus bridged only by altogether evanescent states.

In contrast with this, the various species of organisms that compose the system of the biosphere, differ much more in structure than in chemical composition. And structure is infinitely variable. This does not mean that there are no equations of constraint at all in the analytical scheme for organic evolution. Such equations are furnished by the laws of heredity and variation. But the equations of constraint here have a certain elasticity. An eagle does not hatch from a duck's egg, but a white duck may very well have a brood of parti-colored ducklings.

Now the law of evolution of physico-chemical systems, under certain conditions, can be stated in very precise and concise, if somewhat technical terms. It is to the effect that the transformations within the system are such that its thermodynamic potential under given conditions diminishes toward a minimum. It was not unnatural, when the thoughts of students of organic evolution whose contacts had been with the physical sciences turned to these problems, that they should contemplate the possibility of

a similar "minimum" law as expressing the law of evolution of an organic system. That in a purely formal way such "law" can be framed for systems having certain prescribed properties characteristic of aggregations of organisms, has indeed been indicated, first by me in 1920,[1] and more lately, in greater detail and perfection by Vito Volterra. But to christen such purely formally derived functions *potentials*[2] is, I think, unfortunate. The word potential is no longer a free candidate for adoption into the language of science. It has long been in use in a definite and important sense, to denote a quantity of the dimension of *energy,* in the strict sense in which the physicist uses this term. And of energy in this exact sense there is no mention or definite implication in the derivation of the minimum functions so far introduced into Volterra's analytical treatment of these problems.[3] There is danger that

[1] Lotka, A. J. "Evolution and Irreversibility," *Science Progress,* 1920, p. 406.

[2] See more especially the following references: Volterra, V. *L'enseignement mathématique,* 1937, pp. 297 to 330, and in particular p. 312; "Comptes Rendus," *Acad. Sci., Paris,* 1936, **202,** pp. 1953, 2023, 2113; 1936, **203,** pp. 417, 480.

The use by R. N. Chapman, *Animal Ecology,* 1926, p. 145, of the word potential in still another sense ("biotic potential") is open to the same objection. Chapman remarks about my use of the term "force of mortality" to denote the age-specific mortality. But this is a different matter. I did not introduce the term; it has long been in use in actuarial writings, and I quite agree that it is not well chosen. It has no analogy to the "force" familiar in physics, and no such analogy has ever been implied by me.

[3] The relations which Volterra deduces, in close analogy to certain important properties of dynamical systems (principle of least action, etc.) arise from the fact that his fundamental *assumptions* lead to differential equations containing the dependent variables in quadratic form. These assumptions are plausible as first approximation to the truth, but are certainly only an approximation, and probably a rough one at that.

This situation is very different from that which exists in the case of the equations expressing the formally corresponding principles of dynamics. These are based on *observations,* with which they agree within the limits of error of the most refined experimental procedure.

What Volterra has discovered is not a property of biological systems, but a property of a certain set of equations, and there is no justification for his calling the result "the principle of least action in *biology.*" It is at most "the principle of least action" (speaking figuratively) in a certain mathematical system.

The fact is that the real biodynamics, the actual energy relation of the

by hastily giving a foundling child this name "potential," we may be tempted to desist from further efforts to find the true heir. We must beware of verbal lullabies that by melodious phrase and soothing rhythm sing to sleep our curiosity.

For when curiosity sleeps, science stagnates.

No, for a fuller understanding of the law of evolution in the biosphere we shall have to look in another direction. We are dealing with aggregates of energy transformers, and this fact must ultimately find appropriate expression in our analysis, which will have to give a proper place to the following principles:

The success of any particular species of such energy transformers may be said to depend on three fundamental factors:

1. The fund of energy at the disposal of the individual.
2. His behavior schedule, that is, the proportion in which he expends his energy on different characteristics.
3. His degree of perfection or expertness in the various activities of his behavior schedule.

With reference to man these three factors may be expressed as: (1) How *strong* he is; (2) How *wise* he is in selecting the purposes to be pursued; (3) How *skilled* he is in achieving these purposes.

In the struggle for dominance among the numerous species of organisms, evidently the victory will go to those in whom the adjustment of these three factors is most adequate *under the existing conditions,* that is, to those who,

organism and of aggregates of organisms—oxidation as a source of free energy, etc.—Volterra's analysis does not touch at all; neither does it reckon with the fundamentally important structural and psychological properties of the organism on which its varying success in gathering energy and warding off injurious influences depends.

These remarks are offered with full appreciation of the great elegance of Volterra's analysis, and of a certain very real interest which attaches to it. But it does not go to the root of the matter, and must not be allowed to create the impression that the fundamental problem of biodynamics is solved thereby. It is not even touched.

in their capacity as energy transformers, most effectively guide the energy at their disposal into channels favorable to the survival and growth of the species. It is from this angle, with our eye on the collective effects produced by the totality of component units of the system, that we must study the relation between the various species and their environment, if we hope to develop an adequate analysis of the evolution of the biosphere, as a system comprising a variety of species of living organisms.

But for us the chief interest is in the human population. Now in the human species the development of the three prime factors of success has taken on a very special form. As to the first, it will suffice here to note that by drawing into his service engines driven by extraneous power, man has immensely increased his effective strength, as partially evidenced by the figures which have been cited regarding the quantities of carbon dioxide discharged into the atmosphere. By devising ingenious instruments and machines man has immeasurably multiplied his skills. That these instruments and machines are functionally and in that sense literally extensions of man's own native sense and motor organs has long been understood and often commented upon. But even now probably few persons realize how far the partnership of physiologically compounded soma and manufactured machine extends. It is true that we make machines, but it is equally true, though less generally realized, that machines make us. The reason for this blindness on our part is that the individual steps by which we make our machines are plainly in sight, whereas the participation of the machines in the operations of which our body is the end result is less obvious, because widely and diffusely distributed in space and time. Somewhere in this hand that I hold up to view, I doubt not, are some atoms of nitrogen that have been brought here on steamships from Chile. Gasoline tractors, steam locomotives, mechanical harvester, and I know not what other mechanical devices have played their part in bringing about the

metamorphosis of which my body is the end result. But though I know these facts, they are not vividly in my consciousness, because they are not directly observed.

As for the physiological origin of the body of organisms, often contrasted with the seemingly very different mode of origin of man-made machines, it is to be noted that the somewhat spectacular circumstances that surround procreation and birth, cause us to give too much emphasis to the role of these events, overlooking the more prosaic, but really more essential contribution of the act of feeding. The population grows not by births, but by assimilation. A birth merely provided one more mouth for the entry of food. Not births, but eating makes the man. And if he is to eat, he must have an adequate supply of food brought to him—in the modern community by a whole army of machines.

But machines do not only make men, they very largely make themselves—a point commonly overlooked by those who find amusement in listing properties supposedly characteristic of living matter. A given machine A may not produce another machine A, but a battery of machines $A + B + C + \ldots$, our total industrial equipment, does reproduce its like,[1] namely another battery $A + B + C + \ldots$ What is more, if by some catastrophe the entire existing industrial battery were suddenly wiped out, its reconstruction would be a slow process of evolution, slow for the same reason that organic evolution has been slow, namely because each step forward is conditioned on the prior accomplishment of a necessarily antecedent step, improvement being effected by a cumulation of often small advances.

It may be remarked that our machines still require hu-

[1] These are the facts in the case. The matter could also be approached from the theoretical, deductive side. It would seem to be a problem in geometrical mechanics whether or not a machine could be constructed that would entirely reproduce its like. So far as I know this aspect of the problem has never been given any consideration. It seems to be related to mathematical group theory.

man supervision. Let us be thankful for that! We are sufficiently their slaves as it is. If they started on an uncontrolled campaign of self-reproduction we migh find ourselves seriously embarrassed.

You may have observed that my remarks so far have related only to the first and the third of the factors of success in the struggle for survival, our immensely increased power and skill. We approach now the consideration of the second factor, so far passed by with mere mention.

The marvelous technological progress characteristic of the modern period is essentially the result of the knowledge that comes to us through our natural senses and through artificial sense organs of our own devising. By organizing the knowledge so gained into a body of science, inductive and deductive, we have still further immeasurably increased its scope and power. *But there is knowledge of another kind.*

This statement may be disputed; therefore I will prove it by an experiment.

I have knowledge of a certain physical event which is going to take place in this hall within a few minutes. Only I know it. None of you, in my audience, can know it, because this knowledge comes to me, not through my senses, but through channels absolutely inaccessible to you.

There is here on my reading desk a physical object, a five cent piece. Within less than a minute it is going to change its position and rest on the floor. (Here the lecturer pushed the coin off the desk and it fell to the floor.)

You will see that I had foreknowledge of a certain change that has just taken place among purely physical objects. Now it is a singular, and really very remarkable fact, that this kind of knowledge, foreknowledge of purposed events, superlatively important as it is in human affairs, can apparently be ignored with impunity in entire fields of science.

In the physical sciences nothing is said of purposes and desires. We are so accustomed to this attitude of omitting

all reference to these from the discussion, that ordinarily we do not even notice their absence. But even much of economics can be treated without reference to purposes or desires. Cournot, in his classic *Researches into the Mathematical Theory of Wealth*, remarks that desires which do not eventuate in a purchase or sale do not figure in economic transactions, and need not therefore enter into his discussion.

Cournot's point of view, which thus excludes from economic considerations the "intangible" human purposes and desires, has a strong appeal for a certain type of mind, and for all serious students has an undeniably great interest. But some of us feel that the term "intangible" poorly describes our desires, and we are impressed with the fact that purposes play too important a role in human affairs to be thus summarily dismissed. We observe, following Herbert Spencer, that a suitably compounded assortment of desires is a necessity for the survival of the species, and that an assortment appropriate under one set of conditions may no longer be adequate when conditions are radically altered. The revolutionary changes that have taken place in our powers and skill to attain our purposes may have badly thrown out of balance the assortment of purposes itself. In our present state of society our demands for the prime necessities of life are so easily satisfied that a large part of the time available for labor or work is, in ordinary times, devoted to the manufacture of more or less definitely luxury goods. This gives rise to certain special problems of which we are keenly aware today. First, the mere fact that so much of the working time can be devoted to the manufacture of commodities that are not indispensable, affords the opportunity for periods of relative inactivity, without any serious disturbance of the supply of necessities. This in itself would be no hardship, if in times when such a situation arises, the relative leisure thereby occasioned were evenly distributed. Actually, we know only too well that such even distribution does

not take place, that the result is a state of affairs when a section of the population is busy and another section is idle. Linked with this problem is another one. The demand for the prime necessities of life, food and clothing, is relatively inelastic. After man has been well fed his desire for further food is, to say the least, much diminished. The demand for luxury goods is much more elastic. The man who possesses one automobile not infrequently feels that a second one would be a convenience. Nevertheless we generally suppose that for luxury goods also the law of diminishing returns is still at work, even though less pressingly. Actually, perhaps the principle more often applicable in this field is that of the French proverb "l'appétit vient en mangeant." And the result of surfeit is indigestion.

But where the dislocation of our sense of values, relative to the exigencies of modern conditions, is most acutely in evidence, is in its relation to the perpetuation of our species. Once reproduction is brought under arbitrary control, superlative importance is lent to the desire for progeny. Statistics, correctly interpreted, show only too plainly that in this most important of all adaptive urges, modern civilized populations are deficient. And there are other maladjustments of human aims and endeavors, which, in the present state of man's powers, destructive as well as constructive, threaten to rock the very foundations of civilized humanity.

Is there any remedy for this imbalance between our skills and our tastes; between our knowledge how to do, and our lack of wisdom in choosing what to do?

The remedy is not likely to be easily found, and that for a definite reason: knowledge gained through the senses is fairly easily passed on, also, through the senses. But about the tastes there is something incommunicable. As Pareto has put it, if a man does not like spinach, you cannot prove to him, as you would demonstrate a theorem in

geometry, that spinach tastes good. The same principle applies in more weighty issues.

There are, nevetheless, ways of influencing tastes, of implanting desires, but they are not the ways by which ordinary knowledge is imparted. One method is by the administration of drugs and extracts. Our interest in this method has in the past been mainly negative, its application restricted to the control of the sale of narcotics and other harmful drugs, to say nothing of the recently collapsed "noble experiment." In its positive application, modern developments give hints of possibly surprising effects, which we will not here attempt to appraise.

The second method of influencing desires is that of *suggestion.* Where concerted action is to be taken by a group of people, their desires must be oriented, as the molecules of a bar of steel are oriented to give it magnetic polarity. Something of this kind, for the revision of our standards of value, seems called for by the present state of the human race.

I have sketchily covered a somewhat wide and varied field, thinking that this would prove of greater general interest than a more searching analysis of a narrower problem. Let me in conclusion briefly summarize those points which I should particularly like to leave in our minds.

Man has grown exceedingly powerful. Of that the billions of tons of carbon dioxide which his population discharges into the atmosphere is, if not a measure, at least an indication.

He has gained in knowledge and skill at a rate which in modern times can be likened only to the ascent of a skyrocket. Of this also a numerical index can be given. On the diagram, Fig. 4, abscissæ are calendar years; the ordinates represent the corresponding number of pages in Darmstaedter's catalogue of outstanding events in science and technology.

Thus we have grown marvellously powerful and clever in our means for achieving our purposes. But in the wis-

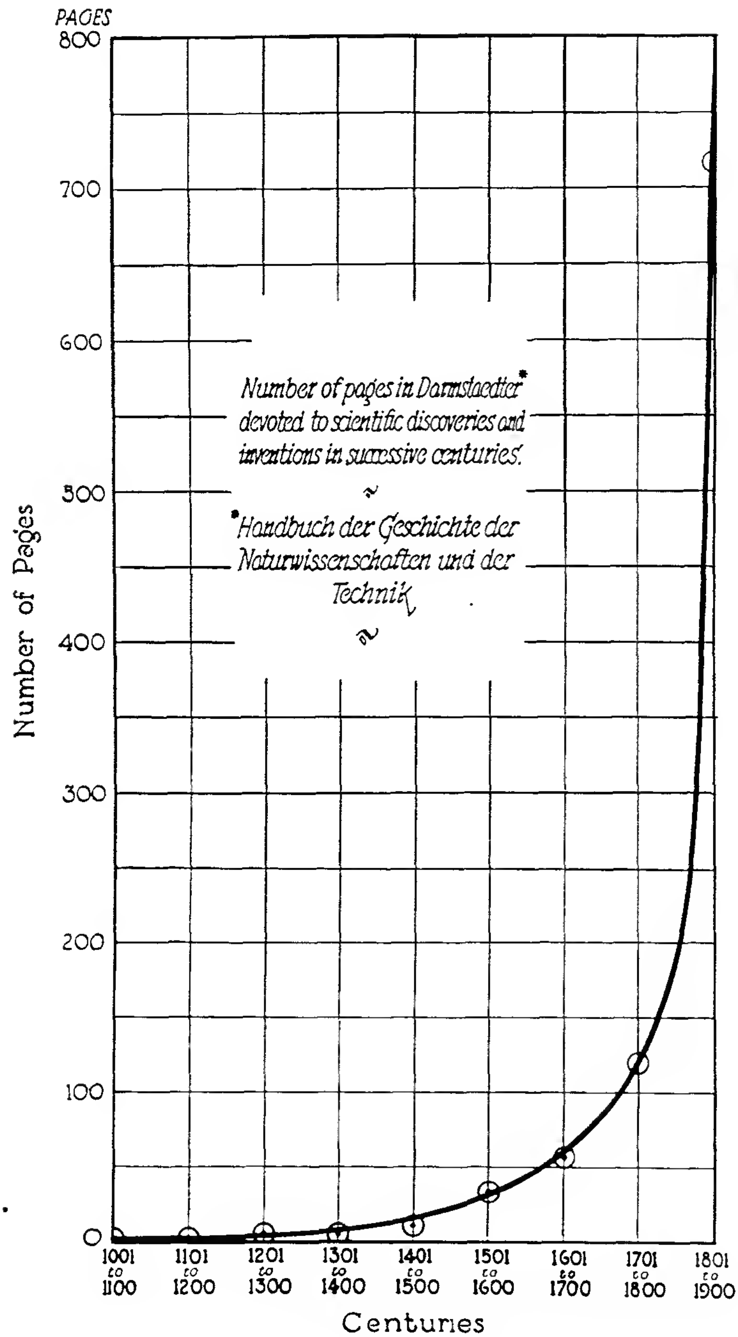

FIG. 4. Growth curve of human knowledge.

dom of our choice of purposes to be achieved we have at best marked time; when, with the advancement of our powers for good and ill, the problem of adjusting our standards of value to the new conditions has become superlatively urgent.

We may well ask: Has an immature human race stumbled upon a dangerous toy? Are we children playing with a loaded gun? Shall we grow up in wisdom before tragedy engulfs us? Is this the riddle of the Sphinx, to fail in which means destruction? Or may we perhaps look forward to an era of an awakening in wisdom, commensurate with the rocket-like ascent in knowledge? Should this come about, then Utopia, from an idle dream, would become a real presence.